This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

QE
R884

The Branner Geological Library

LELAND·STANFORD JVNIOR·VNIVERSITY

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

VOL. VIII.

MDCCCLXXI.—MDCCCLXXIII.

STANFORD LIBRARY

PUBLISHED FOR THE SOCIETY BY

JOHN SMITH AND SON,

70 ST. VINCENT STREET, GLASGOW.

MDCCCLXXIII.

ST

212915

GLASGOW:
PRINTED BY BELL AND BAIN, 11 MITCHELL STREET.

STAMFORD LIBRARY

CONTENTS OF VOLUME VIII.

	PAGE
Opening Address by the retiring President, James Bryce, M.A., LL.D., F.G.S.,	1
The Astronomy of the Nineteenth Century,—Inaugural Address by Professor Grant, M.A., LL.D., F.R.S., the President-Elect,	39
Comparison of the Vital Statistics of the Different Districts of Glasgow, for the Two Years, 1869 and 1870. By Mr. Alexander Scott,	78
Biogenesis, as bearing upon Decomposition, Putrefaction, and Decay. By Dr. Robert Bell,	101
On the Drainage and Sewerage of Towns. By Mr. John Phillips, C.E., London,	112
On Pressure Logs for Measuring the Speed of Ships. By James R. Napier, F.R.S.,	146
On the Results of the Recent Solar Eclipse. By Dr. T. E. Thorpe, F.R.S.E.,	160
On the Analysis of Chrome Ore. By John Clark, Ph.D.,	174
On some Experiments with Fehling's Copper Solution. By T. L. Patterson, F.C.S.,	177
On a Double Sulphide of Gold and Silver. By M. M. Pattison Muir, F.C.S.,	184
On the Action of Dilute Saline Solutions upon Lead. By M. M. Pattison Muir, F.C.S.,	184
The Jargons and Zircons of Ceylon. By M. H. Cochrane, F.C.S.,	186
On the Separation of Phosphoric Acid, Ferric Oxide, Alumina, Lime, and Magnesia. By Thomas Robertson Ogilvie, F.C.S.,	189
On the Part which Ferric and Aluminic Oxides play in the Manufacture of Superphosphate, and on the Comparative Value of Mineral Phosphates. By T. L. Patterson, F.C.S.,	194
On some Evidences as to the very early Use of Iron. By Mr. St. John Vincent Day, C.E., F.R.S.E.,	235
Volcanic Vapours of Mount Vesuvius. By Mr. James Anderson,	268
Livingstone Relief Expedition, *via* the Congo. By Mr. James R. Napier, F.R.S.,	278
On Flour-Mill Fire Explosions. By Stevenson Macadam, Ph.D., F.R.S.E.,	280
On British Weights and Measures. By Mr. James R. Napier, F.R.S.,	288
Discussion on Mr. Napier's Paper,	307-329
On Numeral and Metral Systems. By Mr. E. L. Garbett,	329
On the Purification of Water. By Professor Bischof,	357
The Transit of Venus in 1874. By Professor G. Forbes,	373

PAGE

Examination and Defence of the Principles Enunciated by Mr. Lowe in his Letter to the Chamber of Commerce, Glasgow. By Mr. William Miller, Banker, ... 394
The Mint Bank-Note of the Future. By Mr. James Anderson, . . 417
Technical Education. By Mr. David Sandeman, ... 421
Discussion on Mr. Sandeman's Paper, ... 431-442
The Bank Acts and the Rate of Discount. By Mr. Stephen Mason, . 443
On the Decomposition of Sulphate of Potash by Nitrate of Soda. By Mr. William Henderson, ... 457
On the Past and Present of Iron Smelting. By Mr. St. John Vincent Day, C.E., F.R.S.E., ... 464
Minutes, including Reports on State of the Society and on the Library, and Reports from the Sections, ... 208, 481
Additions to the Library, ... 227, 498

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SEVENTIETH SESSION.

I.—*Opening Address by* JAMES BRYCE, M.A., LL.D., F.G.S., *the* PRESIDENT.

November 1, 1871.

GENTLEMEN OF THE PHILOSOPHICAL SOCIETY,—In conformity with the practice of past years, I have first to allude to the loss the Society has sustained during last session. In mentioning this painful subject, it is matter of consolation to me that the death of only one member has to be recorded. I refer to that of Mr. Moses Provan, of Auchengillan, which occurred at his residence in this city on the 21st of February last. Though not devoted to any scientific pursuit or special walk of literature, he was an active and zealous member of the Society, and often served on the Council. He had a high estimate of the value of scientific and literary occupations for those engaged in professional or commercial business, and did much to promote their cultivation in this city. It is not right that he should pass away from the midst of us without such permanent record of his useful labours as a notice in the *Transactions* of the Society will secure. Mr. Moses Provan was born at Easter Ledlewan, a small property belonging to his father, in the parish of Killearn, in Stirlingshire, on the 21st September, 1821. He received in his native parish an excellent education, which included so much of Latin and Greek as to enable him in after life to enjoy the literature of these languages. Of a thoughtful turn of mind, and being fond of books, he first sought employment with Bailie M'Leod, bookseller in this city. In this situation he had many opportunities of cultivating

literature, and of acquiring that knowledge of books, which he afterwards turned to good account in many ways. He continued in this situation only two years, and then entered the counting-house of Messrs. D. & A. Cuthbertson, Accountants, who had the management of several large estates, and were the promoters of many of the old Tontines of Glasgow. Of this firm he ultimately became a partner; and on the death of Mr. Donald Cuthbertson, about five years ago, the whole responsibility of managing the large and prosperous business of the firm devolved upon him.

Being eighteen years old when he entered the office of this firm, and finding how useful mathematics would be to him in his profession, he devoted his leisure hours to the study of mathematics and the physical sciences, and on two occasions obtained prizes for Essays on Astronomy, publicly offered by the Gorbals Popular Institute. Of this Institution he was afterwards for some time Secretary. In 1845 he became associated with Mr. Robert Reid, Manufacturer in Queen Street, in the formation of an Educational Institute, the credit of the first idea of which belongs, we believe, to Mr. Reid. The name was afterwards changed, at the suggestion of Mr. Provan, to that of the Glasgow Commercial College; the express object of which was, as set forth in the prospectus, to supply the want of a University education to young men in business. A number of gentlemen of note were engaged to deliver lectures, and considerable success attended the movement, the number of students in the first two sessions being from 100 to 120. The want of a building which they could call their own, and of a library for the use of the members, being very much felt, the present Athenæum was formed, at Mr. Provan's suggestion, the Commercial College being merged in it. The efforts of the Committee, sustained by the indomitable energy and perseverance of Mr. Provan, were now directed to attain these objects; and by the liberality of the public they were successful, though the year 1847, in which the effort was made, was very unfavourable, owing to the commercial difficulties of the time. It was a just and graceful tribute on the part of the Directors, to his great merits, to acknowledge Mr. Provan in their first report as the Founder of the Athenæum, and to elect him a member for life. Mr. Provan's acquirements as an antiquary and linguist, his knowledge of mathematics and astronomy, and his acquaintance with books, were now extremely useful in connection with the formation of a library, and the institution of classes and lectures. In the important objects of the Athenæum he took an active interest to

the last; and when the classes were affiliated with the Society of Arts he did all in his power to promote their success. He accepted the office of Secretary to the Local Board, and worked zealously to promote the success of the scheme. Many of the students in these classes, and younger members of the Athenæum Society, speak in terms of warm gratitude of the judicious advice and timely encouragement which their efforts towards self-improvement and independence received from Mr. Provan's genial friendship. When the scheme of Public Lectures for the benefit of the working classes in various parts of the city, and in the neighbouring towns, was organized, ten or twelve years ago, he lent it an active support, and often delivered lectures himself. He gave also a zealous support to the Volunteer Movement, and held for several years a Captain's Commission in one of the corps. The respect and esteem in which he was held by his fellow citizens are amply shewn by the marks of their confidence which he received. He was appointed one of the Commissioners to the Great Exhibition in 1851, and he long held the office of Secretary and Treasurer to the Royal Asylum of Gartnavel, besides other less important offices. On the death of a relative, in 1865, he succeeded to the estate of Auchengillan, which had belonged to a branch of the family for considerably more than a hundred years. In October last he was married to Miss Elizabeth Grieve, daughter of the late Mr. Robert Grieve, town clerk of Dumbarton, only four months before his untimely death. As no issue has come of this marriage, the estate falls by his settlement to his brother, Mr. James Provan, Accountant in this city. Mr. Provan had an interesting collection of coins, medals, rare books and manuscripts. Among these is the only known existing Return made by Robert Burns as an exciseman; also, a report on Burns by his superior officer; and the original charter of Auchengillan, granted in 1631 by Earl Montrose, and attested by Graham of Claverhouse.

PROGRESS OF SCIENCE.

When I had the honour, Gentlemen, of addressing you from this chair at the opening of last session, I called your attention to the progress of science in several departments, more especially in that of astronomy, on account of the absorbing interest of the discoveries then quite recent, the notices of which were either scattered through various journals, or not then published. I propose now to offer a few remarks on some of the present aspects of geology.

ANTIQUITY OF MAN.—This is the first topic which I shall notice. You are all aware of the origin of the late discussions on this subject. First of all, the worked flints in the river gravels of the Somme; and more recently, the works of man of various kinds under stalagmite in certain caves. Besides this, much was made, at the time Sir Charles Lyell's work on the subject was published, of certain skulls said to be of feeble type found in caves on the banks of the Meuse. The evidence from these is now known to have no value. But there can be no doubt that human remains in many of the caves are associated with the extinct mammalia, mammoth, rhinoceros, cave lion, cave bear, hyæna, and others; and at Amiens and Abbeville, in the Somme valley, flint arrow heads with these same fossil species. The number of flint arrow heads in the Somme valley is prodigious; they are to be had by the bushel; in fact, a system of manufacturing has gone on upon a great scale—the flints being thrust cunningly into the gravel and exhumed when the collectors go about. Directed to the best localities by M. Boucher de Perthes, who was at the time in infirm health, and died two months after, I spent a considerable time in searching the gravels for these worked flints, but was not able to obtain a single specimen certainly worked; but they were offered for sale in great quantities. One man, from whom I bought some, told me he could find for me, he thought, something much more wonderful; and in a short time, by a few blows with a pick-axe, brought down a brow of gravel and clay from which a human skull rolled out. Of course it had been previously introduced. I mention these cases merely to shew that too much has been built upon the quantities of these remains. Then, again, the gravels have been assigned to two ages; but the latest researches shew that all the valley deposits are really of one age. There is, then, as regards both these deposits and the caves, the association of the extinct mammals with the works of man; and this may be explained on the supposition that we have merely made a mistake in our former estimates in regard to the epoch of the extinction of these great quadrupeds. It may have been a much more recent event than we once supposed; and it certainly occurred at different times in different countries. The elephant and reindeer were cotemporary in Scotland. Their remains were found associated in a fresh-water deposit at Kilmaurs, under marine sand with shells, over which is boulder clay fifty feet thick. The glacial epoch in which this clay was formed most probably destroyed the mammoth; but the reindeer survived, and remained in Scotland down to the dawning of the

historic period. The gigantic elk (*cervus megaceros*), whose remains are found in Ireland, the Isle of Man, and England, is believed to have coexisted with man—marks of his weapons have been observed upon the bones; and the other great quadrupeds may have in like manner disappeared at a period comparatively recent. With the Somme-valley flints no human remains have been found—only these worked flints; but in the Belgian caves human skeletons, complete except the head, jaws with the teeth, and separate skulls, have been discovered; but they differ in no respect from the present types of skull and bodily form generally. In caves in England, remains of flint knives and pottery have been found in the earth under the stalagmite, forming the cave's floor; but no human bones. There are two difficulties here,—the mode of introduction of the remains and the deposit of the stalagmite. The remains must have been floated in by water; and the apertures are so high above the present level to which the stream could possibly rise, that the excavation of the valley has been considered to have taken place since the remains were introduced. This, however, by no means necessarily follows. I know valleys in Arran which were certainly excavated before the glacial period, inasmuch as they are filled with vast glacial deposits, backing up against a solid rocky face, which formed the old river bank before the boulder clays were thrown down. The geologists of the Ordnance Survey have made known a similar case in Berwickshire; and the Rev. Thomas Brown, of Edinburgh, in a very able paper published in the *Transactions* of the Royal Society of Edinburgh for last year, has given some like cases in Perthshire. The excavation of many valleys may thus have been effected before the cold set in. The melting of the ice, especially if it took place suddenly, would give rise to a greater quantity of water than the channel was fitted to discharge, and so the stream might rise to such levels that an entrance would be given to debris and bones into clefts high on the river bank. Mr. Brown has forcibly described the conditions; and some years ago I pointed out, in regard to Arran, that the valleys of the island bear now no relation to the tiny burns which traverse them, but represent the work of much more powerful agents. It must, however, be remembered that there is no evidence for the existence of man before the glacial epoch. Such an antiquity is not claimed for him; and while this explanation will account for the transport of many of the animal remains, it will not meet that of the human remains, unless we suppose that the ice had not yet fully disappeared from some places when man first

migrated from Southern Europe, or other more genial clime, into the central and northern parts before ice-clad; lat. 40° N. being about the extreme limit of the ice. But this is highly improbable; and another origin must be sought for the introduction of the pottery and worked flints. The only real difficulty seems to me to lie in these being below a certain thickness of stalagmite, which would require a long period to accumulate. But, in fact, we have no correct means of estimating the rate of such deposits; and the rate in one place, if known, is no measure for that in another. It is even quite possible that the pottery may have found its way underneath after the deposit of the stalagmite. Too much is built upon such cases; extreme caution should be used in reasoning, when we have only one or two cases; and we ought to profit by our past experience in regard to the abandonment of theories rashly formed.

With respect to the case of the worked flints and extinct quadrupeds of the Somme valley, I am not prepared to offer any theory from my own examination, and will therefore only refer to the late researches of Mr. Tylor (*Jour., Geol. Soc.*, 1868). He contends for the existence of a "pluvial period," one of much rain, as having followed the glacial period, during which occasional floods of great height would occur, as they do now in certain parts of India, where a rise of 80 feet is not uncommon; and cases of transport occur even more surprising than those of the Morayshire floods, described by Sir T. D. Lauder. Mr. Tylor regards all the gravels as of one age, and to be very recent; not older than shortly before the dawn of history. It is, moreover, quite possible that changes in the relations of land and sea, and of river drainage, may have taken place at a very recent time, and yet be unrecorded. Even Julius Cæsar passes without notice the volcanoes of central France, among which he was long encamped. No one would think of putting back to a time indefinitely remote the case of the canoes found within our city—some at 12 feet above the level of the highest tides; while it has even been contended that the last elevation of land all over the west of Scotland—giving us the 25 feet terrace on which most of our watering places are built—took place since the Roman period.

The case of the stone hatchets found in the Laterite formation of India appears to me more surprising and difficult of explanation. The laterite is a formation of indurated gravel which covers large tracts of Western and Central India, and occurs in patches towards Madras. It is in many places of great thickness; ascends high

upon the mountains; spreads widely over the level plains of the Deccan Plateau; is denuded from valleys, and caps opposing hills at the same level. Though the newest of all the formations, and composed almost wholly of the rocky fragments of the locality, it is unquestionably an old formation, laid down under conditions of physical geography considerably different from the present; which thus imply great relative changes in the levels of the surface and their relation to drainage. In the eastern patches of this laterite, which are considered to have formed the bottom of a shallow sea before historic times, some of these stone implements have been found; but it does not seem to have been clearly made out in any case that the implements may not have been washed in from the surface, or deposited by the hand of man, or recently by other means, in the places where they were found. Judgment in regard to them must therefore be suspended. And, in regard to this whole inquiry, we have meanwhile to rest upon the fact that the remains of man and his works are found only in the uppermost beds, and associated either with recent species or with those which may have become extinct at a very late period.

But there are other than purely geological elements in this question. It is considered by many that the received chronology gives us a period too short for the development of the two or three races of mankind from a single pair—for from such even Mr. Darwin allows all the races are descended—and for the formation of the various languages, as we know them from the first written records. Egyptian sculptures, said to be three thousand years old, shew us Persian, Jewish, and negro features. Five hundred years B.C., the black skin of the Ethiopian was proverbial; and the features, whole physique, temperament, and intellectual character of the Jews have not changed since the time of the Kings: they are still a people perfectly distinct from all others, though scattered among the nations. Already, then, at a very early period, the existing differences had sprung up and got established. Of their origin we are profoundly ignorant. We know only that external conditions, as climate and food, are factors; and we may suppose that varieties suddenly starting up were continued, as in some rare cases in Europe of a black offspring from white parents; as the six-fingers of the Kelleia family, and the short legs of the Ancon sheep, both peculiarities being transmitted; while, by the family or tribe living apart and marrying among themselves, a distinct race became perpetuated. The effect of climate is seen in the most typical negroes being those of the level plains and hot river deltas; the

most abnormal, those inhabiting high plains, who approach the fair race, and lose the crisp woolly hair; and in the old Egyptian race being a sort of transition from Semitic to Negro. The great central Pamir steppe of Asia, 19,000 feet high, was visited by Europeans, for the first time, two years ago. Here a Hindoo race was found who had the fair complexions, blue eyes, and light hair of northern Europeans. It is well known that European children cannot be reared in India. The Anglo-Saxon race in North America is already greatly modified; and New England would soon rapidly fall away in population, but for the constant influx. The Portuguese in Brazil have so fallen off, both physically and morally, that unless there is speedy reform of morals and mode of life, the nation will totally decline. But instances need not be multiplied. The subject is still very obscure, though there can be little doubt that the races have had some such origin as suggested above, and that such a vast length of time as some have supposed is by no means necessary for their development. Now, the chronology of the Hebrews is differently interpreted,—the Samaritan version makes the interval from the Noachian deluge to Abraham seven hundred years more than our English rendering. The Greek version of the LXX. makes the period 1,072 or 1,172 years more; and this lengthening of the period may perhaps be thought sufficient for the divergence of races, the rise of languages and of fixed civil governments, of art and civilization in Egypt and the East. In any case, it must be remembered that the similarity of the symbols for numbers, and the smallness of many of them, render copying especially liable to error in the Semitic languages; and the credibility of the writings in which they appear should not on this account be impugned. All of these questions must be forborne for future enquiry; which should now be directed chiefly to the structure and relations of the various languages of the East and of the tribes of Polynesia. In regard to the origin of language, those who have given the subject most attention profess themselves wholly unable to propose any consistent hypothesis.*

* Even those who deny to the Hebrew Scriptures the authority of inspiration cannot refuse to admit their truthfulness. The late discoveries among the monuments of Assyria and Palestine have given us independent proof that Abraham and Chedarlaomer, Mesha the shepherd king of Moab, Jehoram and Jehoshaphat, Hezekiah and Sennacherib, are real historical characters.—See Professor Rawlinson's works, especially *Alleged Historical Difficulties of the Old and New Testaments:* a Lecture. Hodder and Stoughton, 1871.

DURATION OF COAL.—Another topic of surpassing interest at the present time is the amount and probable duration of the coal and ironstone remaining in our various coalfields. An estimate had been made in 1865, by Professor Jevons, of the probable duration of our coal, founded on the progressive rate of increase in the consumption, which created much public anxiety; and in the year following a Royal Commission was appointed, with instructions to inquire into the quantities of coal existing at workable depths, or under newer deposits, the quantities consumed for household use and in the various applications of steam, and the waste from bad working or other causes. The Commissioners divided the enquiry, assigning one to each of five committees; and have this year completed the work and published their report. The first volume appeared in August last. This contains a general report on the whole question, from special reports on the subjects assigned to the committees. There are, besides, thirteen reports on "proved" coalfields of the United Kingdom, by eleven mining engineers, by Mr. Prestwich, President of the Geological Society, and by Mr. E. Hull, Director of the Geological Survey of Ireland, and six sub-reports upon probable but unproved coalfields. The remaining reports and mineral statistics, with maps and sections, will occupy two other volumes. The general report is brief, and the substance easily given.

The total quantity of coal in all the known or worked coalfields of the United Kingdom, at depths not exceeding 4,000 feet, is estimated, according to the usual methods employed by geologists and mining engineers, at 90,207,285,398 tons. But from a knowledge of the succession of the strata, the strike and dip of the beds, and other physical phenomena, the geologist can often tell with certainty that coal exists throughout areas where it has never been worked. In this way it has been ascertained that under the magnesian limestone and new red sandstone formations (Permian and Triassic) there must exist, at depths not greater than 4,000 feet, at least 56,273,000,000 tons of coal as yet almost untouched. This raises the estimate of unworked coal to 146,480,285,398 tons. Now the present consumption, production, or "output" of coal amounts for the whole kingdom to 115,000,000 tons; and if the quantity remains the same, without increase in future years, our present available stores would clearly last 1,273 years. But in the known coalfields there are 5,922,000,000 of tons between 4,000 and 6,000 feet in depth, and 1,397,000,000 tons between 6,000 and 10,000 feet in depth; total,

7,319,000,000 tons. Under newer formations, the same depths give 25,841,649,067 tons, and 15,302,741,333 tons, making a total of 41,144,390,400 tons; and this quantity taken with the above, will make a total of 48,463,390,400 tons nearly, below the depth of 4,000 feet. It is doubtful whether any of this can ever be worked; if we suppose it were possible to raise it all, then the above period of 1,273 years would be increased to 1,695 years. But there is one more hopeful feature—hopeful at least in the mind of some eminent geologists. Mr. Prestwich, the present President of the Geological Society, an eminent geologist,—calm, sagacious, and of large experience, though a merchant, and not a professional geologist,—considers that coal beds underlie the great area of the south of England, occupied by the newer formations, as the Green Sand, Chalk, &c., and at a workable depth. Mr. Godwin Austen, also another eminent geologist, supports this view. The late Sir R. Murchison, whose wonderful sagacity in such matters is well known, guided by his own researches on the borders of the Belgian and French coal fields, and by the admirable researches of Mr. E. Hull in central England, has given a decided opinion against the view. In this Mr. Hull concurs. My own experience in the south of England, and in those parts of France where the strata emerge from below the upper secondary series, is entirely in favour of the same view. Belgium I have not examined. The Commissioners have, however, wisely avoided this disputed point, and left all consideration of it out of their estimate.

You will notice that the estimate above stated of 1,273 years' duration for the coal practically available proceeds upon an extreme supposition not likely to be realized. In 1660, the production of coal amounted to 2¼ millions of tons; in 1800, to 10,000,000; in 1816, to 20,000,000; in 1854, to 64½ millions; and in 1869, to 107,000,000. In his work on coal, published in 1865, and founded mainly on the previously published work of Mr. Hull, Mr. Jevons predicted that in 1871 the production would be increased to 118½ millions: it has actually amounted to 115,000,000. Mr. Jevons further estimated that in a century the production of coal would increase to 2,607,000,000 tons, and that the duration of the supply would be 110 years from 1871. Professor Jevons' estimate was an increase of 3½ per cent. per annum for each succeeding year on the previous year's consumption; and he contended that the increase would keep pace with the growth of the population and the industry of the nation. But in reply to these statements, it is urged that the *rate* of increase of the population is

diminishing, and must still further diminish. From 1811 to 1821, the rate was 16 per cent.; from 1861 to 1871, only $11\frac{3}{4}$ per cent.; that the rate of increase of coal used *per head* of the population follows a diminishing ratio like that of the population; that for some late years the increase in our industries has been abnormally rapid, and cannot go on at the same rate; that an arithmetical ratio of increase ought to be used instead of the geometric rate adopted by Mr. Jevons; and that Mr. Jevons' method must be wrong, since it leads to impossible quantities; the results evolved being so prodigious that they must be susceptible of some qualification. Estimating on the principle of the above corrections, and adding for each annual increase the average of the last fourteen years, which is about 3,000,000 of tons, then in 100 years the consumption would amount to 415,000,000 tons, and the duration of the coal would rise to 276 years, or to 324, if it were possible to raise at a profit the coal below 4,000 feet. A third estimate is founded upon certain calculations of Mr. Price Williams, which would give the population, upon the above principle of diminishing ratios, at 131,000,000 in 360 years, and the total home consumption at 612,800,000 tons; and allowing the same quantity for export in each century—namely, 1,200,000,000 tons—our whole coal supply would be exhausted in 360 years. If the deep coal was made available, this period would be increased to 433 years.

Now, in regard to all of these estimates, there is much uncertainty. It is most improbable that within our narrow borders we shall ever be able to sustain a population at all approaching to 130,000,000, and that our industries will increase in future years as they have in the last two or three decades. Other nations are overtaking us—some indeed have passed us already; and unless capitalists and workmen learn to get on together more harmoniously, some of our most boasted industries will find new seats, and a gloomy stillness succeed to the busy hum and the resounding hammer. Or, to take a more hopeful view of our position, the progress of scientific discovery may furnish us with new motive powers—such as an electro-magnetic machine—or steam may be generated by the combustion of oil. It seems certain, too, that as other nations advance, their mineral resources will be developed, and there will be less demand for our coal out of our own country. All these considerations may lead us to see that there is no cause for alarm. It is satisfactory to know from the report that much more attention is now paid to the reduction of waste to a minimum; and there will be a constant improvement

in this respect, though in some places it still amounts to a loss of 10, 20, and even 40 per cent. of the whole. I am informed by Mr. William Moore, Mining Engineer in this city, and Mr. Gavin Addie, that in Lanarkshire the proprietors are much more particular than formerly in providing against waste, in the leases which they have granted of late years.

In regard to the future availability of the coal believed to exist below 4,000 feet, the Commissioners are by no means sanguine. There are two difficulties to contend with—the cost of sinking shafts and of raising coal from such a depth, and the high temperature which prevails. Omitting the first 50 or 100 feet of the earth's crust, so as to be clear of all effect from the penetration of solar heat in past ages, it is found in all countries, tropical, temperate, and polar, that the temperature of the rocky faces in a mine and of the water issuing from clefts increases at the rate of about 1° F. per 50 to 60 feet in different places: in Scotland 54 to 55 feet; in many English mines a little more,—and taken for all at 60 feet by the Commissioners, in order to present the most favourable view for deep working. Now, the difference between spring water and boiling water being 162° F., boiling water will be reached between 8,910 and 9,720 feet of depth. This increase of temperature is the only obstacle to the working of mines which the Commissioners consider, and they have collected much valuable information in regard to it. It is found that a good deal depends on the hygrometric condition of the air, the exhaustion and distress being much less at the same temperature when the air is dry. Now, the dryness is greater in deep than in shallow mines; but the air, even in the driest, is more than twice as moist as that to which the men are exposed who work in Turkish baths, of which the temperature often passes 110° F. The Commissioners consider that when the temperature passes a little above 80° F. it becomes detrimental, by reducing the endurance or force of the men, and obliging them to work shorter time; and they express the opinion that labour cannot be carried on except for very short periods in moist air at or near blood heat (98° F.) In dry air it would be possible for a few degrees higher. Now, at 3,000 feet the earth will have a temperature of 98° F., but the air in the passages is about 7° F. lower than this; so that at 3,420 feet the air would have a temperature of 98° F.; at 4,000 feet, 150° F.; and at 10,000 feet, 215° F. Now, the deepest mine in the United Kingdom is that of Rosebridge, near Wigan, of which, you may remember, Mr. Hull gave an account to our Society (see *Trans., R. S.*, 1870).

Here the depth is 2,425 feet, and the temperature at the bottom 94° F., and rate of increase downwards after the first 100 feet, 1° F. for 55 feet. In Scotland the deepest mines do not pass 1,100 feet, so that there is still a considerable superior limit to the temperature of possible work. Taking the most favourable view of the action of all the ventilating processes known to us, the Commissioners express the opinion that, so far as their present knowledge enables them to judge, 4,000 feet will be about the lowest limit of possible working. Theoretically, however, it is a question merely of ventilation. If there was a plentiful supply of cold air in the working passages a man might dig out a stratum of a red heat. We know what men can do in glass-houses, and stoke-holes of engine-rooms, where distress and exhaustion are speedily repaired by an access to fresh air, and there is no injury to health. The Commissioners recommend the more general introduction of coal-cutting machines. These are worked by compressed air, conveyed into the mine by pipes; and if the air is thoroughly cooled after compression, its escape in the mine will, by its expansion or rarefaction, absorb a large quantity of heat. Experiments directed by the Commissioners shewed that, by using one machine, this expansion reduced the air to 7° F. below freezing, and lowered by 1° F. the whole body of air passing a working face. Now, one working face may employ seven such machines; but the cooling effect would not increase in proportion, because there would be quicker absorption by the greater reduction of temperature; and if the volume of air was increased by giving the current a greater velocity, the cold air of the machines would have a less effect of dilution upon the warm air of the passages. The difficulty also increases greatly with the depth; for in deep workings there must be a larger underground area and longer passages in order that the outlay on deep sinkings, which is enormous, may be met. And further, if the number of machines must be limited by the circumstances of each working, it is plain that the expense of sending down cold dense air merely to escape would imply great cost, and waste of power unless its expansion could be used as a force to raise the materials to the surface, or to drag the waggons to the bottom of the shaft. There is yet another difficulty connected with the more general introduction of these machines: they are not adapted for thin seams, of which we have many in our Scottish coalfields, and they are much more liable to break with us than in England, on account of the frequent occurrence of whinstone rock. Mr. Gavin Addie informs me that they are

very little used as yet in Scotland; and are not likely to be introduced largely for the thicker seams until wages rise very much higher than they are at present. Their general use, if it were possible, would secure a great saving of coal, and enable men to work for a longer time; since the miner's heaviest work is the use of his pickaxe in the warm atmosphere, and since by chipping away the edges of the opening, to use his axe freely, he must necessarily produce a great quantity of small coal, which is lost as dross.

The report on which the Commissioners rest their calculations for Scotland has been made by the well-known engineer, Mr. John Geddes, of Edinburgh and London. He estimates the entire available quantity for the whole of Scotland at 9,843,465,930 tons; of which the county of Edinburgh has 2,153,703,360 tons; Lanark, 2,044,090,216 tons; Ayrshire, 1,785,397,089 tons; Dumfriesshire, 358,173,995 tons; and 1,800,000,000 tons are under the waters of the Frith of Forth. As the vend of coal in Scotland for all purposes is 12,000,000 of tons, reckoned by the returns of 1866, the total quantity above named will last 820 years, on the supposition that the entire output will remain at the production of the present time,—a supposition which cannot be admitted. Besides this, exception may be taken to some of Mr. Geddes's figures. He seems to me to over-estimate greatly the quantity of coal contained among the oolitic beds at Brora in Sutherland, and of that contained in the small and imperfectly developed basin at Campbellton: for the first he sets down 3½ millions of tons, for the other 7¼ millions; but this is of little material importance, as being only a year's supply, and as the Commissioners have thrown Mr. Geddes's Scottish figures into the general sum, and estimated the duration of the whole at 276 years. The estimate for Lanarkshire seems also much too great.

It ought to be a matter of comparative ease for a mineral surveyor to estimate the quantities of coal in the Scottish fields, since the surfaces are everywhere exposed and the seams well known. We have no newer formations under which the seams could extend, the great basin or trough, the greater axis of which reaches from Ardrossan to St. Andrews, being bounded on both sides by the Old Red rocks which rise up from under the coal; the red sandstones of Dumfriesshire, said to be New Red belong to the English system, being continuous with those of the Eden valley. The red sandstone of this age, properly Scottish, is that which occurs in small patches near Ayr, is of Permian age, and may conceal some coal beds of insignificant extent.

A few years ago, when publishing a new edition of my "Outline of the Geology of Clydesdale," I consulted Mr. William Moore, Civil and Mining Engineer, in regard to the quantity of coal still remaining in our central fields; and I felt satisfied at the time that the estimate by Mr. Moore, which I then gave, was made on a correct principle. I have again consulted Mr. Moore; and his opinion is, that the quantities of coal and ironstone then given were not under estimated—namely, 424,620,700 tons of coal, and 72,081,400 tons of ironstone. Seams which had a less thickness than 18 inches were not taken into account as supply, because they could not be worked profitably at the actual depths. The estimate of the duration of the central fields of Lanarkshire, made at that time, was about 150 years for the coal and 70 to 100 for the ironstone. But the works would be stopped before the completion of these periods; for the deep workings would entail such an addition to the price that the Continent, and probably America, would be able to compete with us by sending us both coal and pig iron. The resources of the United States and their facilities in regard to markets are rapidly developing. Their coal is inexhaustible and easily worked; and they will soon be able to produce iron as cheap as, or cheaper than, we are now doing. So that, whenever our cost of production rises by scarcity of coal or ironstone, and by increase of wages, to 17s. or 20s. per ton above the actual price, the United States will come to our markets, since the freight will not exceed this cost. If, then, our coal was 18s. per ton, our pig iron could not be produced for less than 90s. or 94s. per ton; while with all their appliances, the United States could produce it for 50s.—the average cost at present in England; in which case there would be a large profit, even with freights at 20s. per ton. It is a remarkable feature in the iron trade, as shewing the increased value of ironstone, that thirty years ago the price close to the furnace was 6s. to 7s. per ton. Mr. Moore informs me that ironstone is now annually brought from Spain, and charged from 16s. to 26s. per ton to the ironmasters; that the country has immense iron mines; near Bilbao, where it has the character of a blackband, and on the south-east coast, where it is a pure magnetic oxide; and that within a year these mines will be in a position to send us 400,000 tons annually. It is well to be assured that in case our own supply were exhausted there is an abundant store so near to us, from which iron can be had for a price that will not be excessive.

I mentioned already that something has been done of late years

to stay the waste of coal which had gone on for a long period, and was rapidly tending to its speedy exhaustion. But much remains to be done, as the waste in most places is still wanton and prodigious. No effectual check will be put upon this waste till we have the men who are engaged in the work practically educated; taught the principles of mining; so imbued with knowledge and improved in intelligence that they will feel an enlightened interest in working the mines to the best advantage; and practising those methods which will most conduce to the industry and wealth of their country, by saving and not wasting the stores of fuel and of force committed to their guardianship. This Society has repeatedly urged the establishment of a Mining School, or rather the revival of one, which has gone into decay and been given up. No time could be more favourable than the present for again urging this subject on public attention. There is a proposal before the Senate of the University to divide the chair of natural history into two chairs—geology and zoology. This proposal—generously made by the occupant of the chair himself—is in entire harmony with the spirit of the times. Both subjects are of late much enlarged. Their domains now embrace many topics which were not thought of when the chairs were instituted; and few men can be found who are capable of doing justice to both. Some indeed may be able; but however willing they may be, the occupation with the duties of the two chairs leaves no time for that original work which is expected of a professor occupying so prominent a position. I would, then, earnestly urge that the opportunity be seized upon of separating the chair of zoology from that of geology, and instituting a chair solely for the latter subject, and for connecting the professor who should be appointed to the new chair with a Mining School, either attached to the University or elsewhere. An endowment would of course be required; but it might easily be provided, or at least a large part of it, by the coal and iron masters alone, whose interests it most deeply concerns that our resources should be husbanded. I learn from Mr. Gavin Addie, who knows the general feeling, that coalmasters are desirous of employing the best instructed and most intelligent men they can meet with as superintendents of works, and that men so qualified are always engaged in preference, when there is any choice.

To obtain men thus qualified it is in vain that we longer depend upon the Mining School in London. As regards the provinces, it has failed of its object; from this district at least men have not

gone, nor can we expect them to go, to London. It is too distant and too costly; and though the professors connected with the school are as eminent as any whom the country can supply, and have admirable appliances for teaching, yet a position for a mining school here, at a great centre of manufactures, machine-making, and mining, in a district whose physical structure is well shewn upon the surface, offers advantages far surpassing those of London. The present combination of circumstances gives facilities for carrying out such a scheme which may not again occur; and I feel sure that a united vigorous effort would soon secure the object; and that, by proper representations of its great national importance, the Government would be indueed to provide a part at least of the endowment.

EVOLUTION HYPOTHESIS.

In last year's address I offered some remarks on the hypothesis of the gradual evolution of organic forms from one another, and the origin of all from a simple organic spore. These remarks were made in view of that aspect of the hypothesis presented in the writings of Darwin—the gradual modification of animal forms and adaptations of organs to special uses by what was called "Natural Selection," and that "Struggle for Life" in which those more perfectly adapted to external conditions had the mastery, and the fittest survived. In his *Origin of Species*, Mr. Darwin set forth the leading principles of his doctrine. The proofs were in great part reserved; but were partly made known five years later in a work on *The Variation of Animals under Domestication*, in which a vast collection of interesting and curious facts was brought together in regard to the changes wrought in animals and plants by the conditions which man can establish. The second portion of the so-called "proofs" appeared in January last in a third work, entitled the *Descent of Man*, wherein the facts and reasonings of the former works were applied for the purpose of proving that man had proceeded by regular descent from the lowest form of organic life, by a gradual evolution through the brutes and anthropoid apes, to his present high state of organization. This conclusion had been to some extent foreseen, as regards man's bodily organization, for the reasoning had been all tending that way; but it was with astonishment we learned, now for the first time, Mr. Darwin's views regarding the development of our esthetic and moral powers, our ideas of Right and Wrong, of our whole spiritual nature in fact, from certain sentiments and powers resident in the minds of brutes. There is

thus no longer any doubt where the hypothesis is meant to lead us; the issue is set before us in the clearest terms, and one more momentous cannot be conceived. No subject of science or philosophy can be of equal interest to us as that which relates to our own origin in the past and our destiny in the future. If our descent is from some creature like the larva of the molluscoid Ascidian—the last term but one of the series being the narrow-nostrilled monkeys of the old world—the idea of a divine or godlike prototype is not only a fable but profane; if our soul and spirit are but the faculties of brutes modified, the high sanctions which are attached to our obeying the dictates of our inner conscience, as of a law written in our hearts, and to our sacrificing our own pleasure and happiness for the principle of "the enthusiasm of humanity," are but vain fancies; we are of all creatures most miserable—"Let us eat and drink, for to-morrow we die." "If these views be true," says an able writer (*Ed. Rev.*, July, 1871), "a revolution in thought is imminent, which will shake society to its very foundations, by destroying the sanctity of the conscience and the religious sense; for sooner or later they must find expression in men's lives."

These remarks are not made as any argument against Mr. Darwin's views; for it is not thus they must be met, but merely with the purpose of shewing the magnitude of the issue. It is, indeed, put as broadly as this by Mr. Darwin himself, when he says, "He that is not content to look at the phenomena of nature as disconnected, cannot any longer believe that man is the work of a separate act of creation" (*Desc. of Man*, ii., 386); and, again, "the early progenitor of all the vertebrata must have been an aquatic animal with gills, the two sexes united in one, and the most important organs, as the brain and heart, imperfectly developed. This animal seems to have been more like the larvæ of our existing marine ascidians than any other known form" (*Ibid.*, ii., 389).

Now, in regard to the first portion of these views, it has been long admitted, and was indeed a matter of speculation among the ancients, that man in his outward organic structure is but the last and highest term of a gradually ascending series, and is linked on to the next lowest, the anthropoid apes, by so many resemblances that almost all his bones, muscles, and blood-vessels are nearly the same. Late researches in embryology have made the relations still closer; while the researches of naturalists in many parts of the world have brought to light new forms of which some undoubtedly are links in the chain of Being, before amissing; and the

influence of man in modifying organized beings was never so fully shewn as it has been of late years. All this has conspired in favour of Mr. Darwin's views, and is no doubt the chief cause for the revival in our time of speculations which were advanced before, but with less wealth of collateral adornment, and in a spirit having less the show of scientific calmness. Hence the plausibility and show of reason which these speculations exhibit, and the amount of acceptance they have met with. But the resemblances in question are really only apparent. And, first, as to the embryo; granted that that of the quadruped and of man are in early stages very much alike—it is too much to say they are undistinguishable—are not the organs which are to be formed "in continuance" from this "yet imperfect substance" destined to perform very similar functions in the economy of life? But is it not manifest that there must underlie this apparent similarity, some innate tendency, mysterious and inscrutable, which neither microscope nor dissecting knife will ever reveal; and in virtue of which an elephant or an ape is the product of one, and a human being of the other? Then, secondly, as regards the chain of Being, which is not wholly unbroken, there being many "missing links," it has always appeared to me that it is explicable by the idea of one grand plan which runs through the whole organic world (*Address*, 1870), and has its parallel in the inorganic world, where we see thousands of crystalline forms built up—on purely geometrical principles—of a very few simple shapes, with impressed laws providing for repair. If there be such a thing as a Presiding Intelligence at all—and Mr. Darwin does not seem to deny that there is—it is surely a grander conception of His working, to reckon that He goes upon a uniform plan throughout, varying it for special conditions, so that the life and enjoyment of the creatures placed under these conditions shall be secured; and as the conditions gradually change, so do the adapted organizations. It will be objected to this explanation that it is unscientific, as it introduces other than natural causes. But it has been well remarked (*Times*, April 8, 1871), that "to assume that none but natural causes have acted is really assuming the point to be proved." Without some such overmastering interference, it is impossible to see how a modified organ originates and is adapted to conditions outside of it. The hypothesis of "Natural Selection" gives no account of such an origin; the variations are accidental or produced by chance, and are merely dealt with after they have arisen. This defect in the system was manifest from the first, and felt latterly by Mr. Darwin to be the weak point in his hypothesis. He even acknowledged

that if beauty and ornament could be shewn to be the sole object of modifications, Natural Selection must fail as an explanation. Now, it was shewn in one of the best reasoned chapters of the Duke of Argyll's *Reign of Law*, as a whole, perhaps the most able work which Mr. Darwin's teaching has called forth, that there are numberless cases of the kind—as, for example, the ornamentation of humming birds and the peacock. Partly, no doubt, in consequence of these criticisms, Natural Selection has been declared, in "the Descent of Man," as "insufficient of itself to explain all the facts, as many structures had been overlooked, which are neither useful nor injurious," and the aid of "Sexual Selection"* has thus been called in to supply the requisite explanation. More than three-fourths of the book are taken up with illustrations of this view, drawn from the whole animal kingdom, and given with a liveliness and variety of detail which are most amusing and interesting, in spite of the grim conclusion which is looming in the distance. These portions are not the less readable that there is much which is fanciful; and that that for which there is foundation is overstrained. The result of this wealth of illustration has been, that the main question is examined in too cursory a way, and "the subject treated in an imperfect and fragmentary manner," to use his own words. The analysis of our faculties and their relations to those of the brute creation shews a singular want of ability to grapple with a high ethical question; and the result, as was to be looked for, is vague and feeble. It is impossible to go into a full criticism of this part of the work; and it may only be said that I consider the reasoning to be valueless, as Mr. Darwin proceeds on the assumption that the human faculties and those of brutes are of the same kind. A difference is allowed only in regard to the highest order of faculties, those, namely, of Abstraction and Generalization, which "could not have been developed in man till his powers had been advanced to a high standard, and he had acquired the instrument of a perfect language." But this is plainly reasoning in a circle; the formation of a perfect language, one abounding in abstract and general terms, being solely dependent upon the exercise of these very powers, since plainly the idea in the mind must precede the invention of a term or symbol. Language, even that of the simplest structure, is mainly the expression of relations; and it is precisely such relations among ideas of which the minds of brutes are incapable. This obvious objection to his view is indeed noticed; but it is

* By this term Mr. Darwin designates the advantage which certain individuals of the same sex and species have over others in exclusive relation to reproduction.

dismissed with the remark, that these high faculties need not be discussed, as their nature is not agreed on among ethical writers—a manifest evasion of a difficulty which ought to have been met and explained.

Not less feeble and unsatisfactory is his treatment of the origin of the moral sense—the noblest of all our powers—and of ideas of Right and Wrong—hitherto regarded as peculiar to man, and correlating him with Intelligences of Higher orders. Certain animal instincts do indeed give an impulse to certain quadrupeds and even birds to act as if some idea of right and wrong had a seat within them. But this is wholly different from a tribunal established in the inner consciousness, whereby those who are without a law are a law unto themselves; and further, if such powers have been gradually formed in a being developed from the lowest type of life, this is in reality a purely materialistic aspect of the origin of mind, and one which makes life and spirit no more than mere organization—a view which never had such proof as to satisfy a philosophic mind, and is now given up by most persons. How the variations arise which bring about the changes and developments, no explanation sets forth. "The more complex instincts seem to have originated independently of intelligence, while instinctive actions may lose their fixed and untaught character, and be replaced by others performed by the aid of the free will. On the other hand, some intelligent actions—as when birds on oceanic islands will learn to avoid man—after being performed during many generations, become converted into instincts, and are inherited. They may then be said to be degraded in character, as no longer done through reason or from experience. But most complex instincts have been gained through the natural selection of variations of simpler instinctive actions. Such variations appear to arise from the same unknown causes acting on the cerebral organization, which induce slight variations or individual differences in other parts of the body; and these variations, owing to our ignorance, are often said to arise spontaneously" (*Desc. of Man*, i., 37). Thus the variations in the external organs bringing about modified structures, and those in the cerebral organization, arise alike from "unknown causes." Why then assign them to Natural Selection? It can neither originate a variation in the brain nor any outward organ: and in thus assimilating the two, countenance is given to the view that organization is life. In another passage farther on, an admission is again made which mars completely the force of the reasoning

—that actions in the brutes similar to those of man may arise from causes and motives wholly unlike. Does not this indicate a total dissimilarity of mental constitution and attributes, and lead to a conclusion the very opposite of that which it is attempted to establish in regard to the development of the faculties?

Other cases of inconsistencies and fatal admissions might be instanced,—as when Mr. Darwin allows that animal instincts often fail, and yet are held to have been gradually developed by Selection from minute variations favourable to the individual; and again, when he confounds the social instinct in man—a feeling gradually developed from childhood—with an animal instinct perfect at first; and the growth and development of parts, as in the case of the ear, eye, and larynx, before the developed parts could have been of any use to the animal, and so, according to the doctrine, Selection could not have acted. But, in truth, there is such vagueness, such failure in a right analysis, such an absence of anything like proof, that it seems almost a waste of time to follow the speculation through its details. It must strike every one, in reading the speculative portion (the first half of the first volume), how often a definite conclusion is at length drawn from "possibilities" often repeated, such as,—"it seems certain," "I cannot believe otherwise," "it may have been," and others of the same kind. An example of this is seen in the passage regarding Instinct, quoted above, in which such expressions as "seem to have originated," and "appear to arise," pass into the form of positive assertion, as if proved truths were spoken of. Mr. Darwin makes no distinction between the vivid conceptions of his own imagination and the realities of objective fact. Such a method was in truth inseparable from the treatment of a speculation for which there really is no scientific basis. After all that has been done by Mr. Darwin (and he has done it as well as any one is ever likely to do it—better certainly in many ways, for, as a sagacious, accurate, and patient observer, he could not be surpassed), there are only to be had vague resemblances—certainly nothing of the nature of a scientific proof or logical inference. At every turn the speculation fails; and all persons who think accurately, and are accustomed to reason, will rise from its perusal with the conviction that the case has utterly broken down. Even those who are not able to point out, *secundum artem*, flaws in the reasoning, will *feel* its unsoundness. The most ingenious arguments will prove insufficient to overbear that universal inner consciousness, in which a savage could share, which revolts from the notion that man is a "specialized brute."

We feel that the gulf which divides us from the lower animals is impassable, and can never be bridged over; that we have a nobler pedigree, and "are only for a little time lower than the angels;" that the power and privilege of scrutinizing this great universe to its very depths, and discovering its laws, are a glory and honour which belong to man only—are a crown for him, attesting his dominion; that all things are put under his feet; that all beasts of the field, birds of the air, fishes of the sea, and whatsoever passeth through its paths, are to subserve his wants and minister to his will. This same inner consciousness reveals to us our relations to a Supreme Being, and assures us of a glorious destiny in a higher life; and is to all of us an evidence which no "show of reason" that has as yet been advanced in support of any evolution theory can countervail. Such an argument as this Mr. Darwin would probably stigmatize as an appeal to the feelings or the imagination; forgetting that his whole theory is founded on and wrought out by his own active imagination, and how largely it draws on the imagination of his readers, though certainly no appeal is made to their feelings.

The objections to the evolution theory of a purely physical nature, and those which are encountered when it is regarded in the light of geological discovery, are so numerous and strong as to be of themselves sufficient to condemn it. I shall notice briefly a few of these, without attempting to estimate their relative weight. *First*, then, Natural and Sexual Selection can have operated within very narrow limits, and only along certain lines, since we find the lowest, intermediate, and higher forms of life still existing which had their place through the whole geological series; unless we could suppose a continual renewal at the base from spontaneous generation, or creation, and a continual movement upwards in all. Excepting the few groups which are extinct, and also the slightly different proportions of the groups of the various forms, all things continue as they were at the beginning. *Secondly*, through all the elaborate details of his great book, *Animals under Domestication*, "there is no evidence put forward by Mr. Darwin to prove that any group of animals has, by variation and elective breeding, given rise to another group even in the least degree infertile to the first." I quote the words of Professor Huxley, who "would be glad enough to find a good foundation for the doctrine" (*Essays*, p. 280; *Jour., Geo. Soc.*, May, 1870). Thus the old test of infertility is not yet set aside. Hybrid breeds cannot be propagated, though the

hybrid is fertile with either side, and the old type is resumed in the second or third generation. Species have thus an independence, a permanency in nature; and variation is only within certain limits. No coincidence has yet been established, and the limits of variation are unknown. In regard to plants the same law has force; and with them, as with animals, the old types are re-assumed when the control of man is withdrawn. Therefore, till we have the exact limits of variation, till actual proof is given that variations become permanent, that one species has passed into coincidence with another, there is no argument from similarity of structure that man is descended from an ape. And further, as if actual limits were set, there is even a penalty attached by "provident nature" to the production of these varieties by man. They are liable to diseases unknown in the wild state; and the instinct to protect becomes unnatural, the mother destroying her own young.* Again, there cannot be produced from the living creation, or from the

* I gladly avail myself of the following remarks addressed to me by one who has bestowed much more attention on metaphysical and moral questions than I have been able to give them :—

There seems every reason to believe that it is a law of animal and vegetable life, that development beyond a certain point becomes disease, or affects the constitution so as to generate disease. This is said to have been the case with the ancient athletes. We often see high cerebral development and activity end in imbecility or insanity in the individuals possessing it, or in their offspring. While there can be no doubt that talent (if not genius) is hereditary, there seems to be a limit to its transmission, so that when it attains its greatest brilliancy in one individual, the flame dims in the next generation. There is a well-known mechanical law which limits the size of any machine, living or inanimate, so that its strength and efficiency do not increase in the same ratio as its dimensions. This sets a limit to the *bulk* of animals of any given type; there seems to be a simliar limit to their development in other respects.

There are two principles in the *Novum Organum* which Mr. Darwin systematically disregards :—1. That we ought to guard against what in that work are called *anticipationes*, *i.e.*, rushing to general conclusions without having a sufficient number of facts on which to found our generalizations. Mr. Darwin shews very ingeniously how certain changes *may have* taken place; then assumes that *probably* they did take place; and in a page or two speaks as if there were no doubt that they *actually* did take place. The second principle of Bacon referred to above, is, the great importance of what he calls *instantiæ negativæ*. These Darwin coolly sets aside. Witness his amusing way of dealing with the fact of the decay of intellectual power in the descendants of the ancient Greeks. And he makes no account of the patent fact, that the oldest literary productions extant have never been surpassed and seldom equalled in more recent times.

fossil groups, any animal form which in the least degree serves to bridge over the gulf, allowed by Mr. Darwin himself to be immense, between man and the apes. This difficulty is got over by assuming that such links will yet be found when the first seat of our race has been thoroughly explored. The Catarhine apes, from which we are descended, had their native habitat in central Africa; hence the first seat of mankind must have been there also! There the transition forms, bridging the gulf between man and the ape, are yet to be discovered. No assumption of a character so wild and gratuitous was ever made by a man of science. Mr. Darwin will admit no evidence but what he calls scientific; and such is a specimen of the evidence on which conclusions are built, so important and far-reaching. It was surely here, if anywhere, that evidence equally good as that called "scientific" might have been safely introduced. The testimony of our oldest book—regarded as of no higher than human authority—confirmed as it has lately been

In Mr Darwin's account of the genesis of moral sentiments, there is little or nothing new; it is just the utilitarianism of Jeremy Bentham and the two Mills, only that they suppose these moral sentiments to have originated in man as he is, while Mr. Darwin sees the germ of them in our supposed brute progenitors. But this whole utilitarian theory has been utterly demolished in that memorable controversy between Macaulay and the elder Mill, carried on about forty years ago in the Edinburgh and Westminster Reviews. Though Mr. Darwin treats almost exclusively of the origin of *social* virtues in these speculations, he does not altogether ignore some other moral sentiments. He says (i., pp. 95, 96) that "prudence and the other self-regarding virtues" [temperance and chastity] "which do not obviously, though they may really, affect the welfare of the tribe, have never been highly esteemed among savages, though now highly appreciated by civilized nations. The greatest intemperance with savages is no reproach. Their utter licentiousness, not to mention unnatural crimes, is something astounding." The disregard of facts in this passage is "astounding" indeed. Has Mr. Darwin never read Theocritus, or the Lesbian lyrists, or even the *Anabasis* of Xenophon! Does he not know that the most civilized nations of antiquity not only practised that "utter licentiousness" and those "unnatural crimes" without the slightest sense of wrong or feeling of shame, but embellished them with the charms of poetry, and ascribed them to their deities? And is he not aware that while concubinage, or a sort of morganatic polygamy, is distinctly presented to us in the Homeric poems as a feature of the comparatively rude social condition of early Greece, those poems contain no trace of that promiscuous licentiousness as between the sexes, far less of those unnatural crimes, which were sung by the poets, and coolly discussed without a word of disapproval by the philosophers, of a refined and civilized age. As a late writer in the *Saturday Review* or *Pall Mall Gazette* (I forget which) has shewn, civilization *per se* has never done much for morality.

Mr. Darwin ignores altogether the idea of duties owing to God, a circum-

in many ways, runs parallel with the course of ancient history, to shew that in central Asia, not central Africa, was the original seat of our race. Another instance of the same kind occurs in the account of man's early condition and his first notions of religion. It was essential to the hypothesis of man's descent that his primeval condition should be one of "utter barbarism," and his religious belief one of progression from the grossest superstition of object worship and fetishism, through polytheism, up to the idea of one Supreme Being, which is acknowledged to have a place in the bosom of most savages; and accordingly this hypothesis is the basis of all his reasonings. The papers and works of Sir John Lubbock are the main authority on which he rests; but these works are based upon the same original idea, that man is a development from "something not worthy to be called a man," and are pervaded by the one radical assumption, "that every very ancient custom, savage or immoral, is regarded as proving that this barbarism was the original condition; whereas it shews merely that the nation or tribe had passed through a savage phase, which can in many cases be shewn to be one of declension from a higher state. Neither does it follow that, even if man at first was in a state of childhood as regards knowledge, and had little acquaintance with the industrial arts, he was therefore barbarous, ignorant of duty, and without the idea

stance which is not to be wondered at, inasmuch as he makes no nearer approach to a recognition of God than to say that "the question whether there exists a Creator and Ruler of the Universe" . . . "has been answered in the affirmative by the highest intellects that ever lived." He says (i., p. 65), "There is no evidence that man was aboriginally endowed with the ennobling belief in the existence of an Omnipotent God." What is aboriginally? Certainly the *earliest records we have* of human beliefs present this belief to us in its highest form. He says again (i., p. 182), "The highest form of religion—the grand idea of God hating sin and loving righteousness—was unknown during primeval times." This is mere gratuitous assumption, and is all the more amazing when we reflect that unquestionably the very oldest work, in what Professor Huxley calls the "grand old literature" of the Hebrews, in fact the oldest written production of the human mind that has come down to us—the Book of Job—is occupied from beginning to end in inculcating these very things—viz., the "ennobling belief" in a God of infinite power and wisdom, the Creator and Ruler of the universe, and that He hates sin and loves righteousness. If by "primeval times" Mr. Darwin means times anterior to all literary productions, he of course places himself beyond the reach of direct confutation. But as it is a fact that the farther back we trace man's views concerning God, the loftier and juster they appear, it surely is the opposite of philosophical or scientific to assert without a particle of proof that such views were unknown in the earlier ages into which our researches are unable to penetrate.—*Rev. Dr. Bryce in a letter to the author.*

of God" (Duke of Argyll, *Prim. Man*, p. 132). There is the additional practical objection to the hypothesis that unaided savages do not rise; and that even in our own day, with many aids, it is almost impossible to induce them by example and precept to embrace the arts of civilized life. Such a change indeed is only effected when the principles of Christianity are embraced and practised. This argument has been most ably put in a popular form by Archbishop Whateley.* The Duke of Argyll has, however, shewn that in one respect Whateley has carried his argument too far; but this is not a legitimate objection to its general truth.—But I cannot pursue this subject further; and must refer you to the works which I have named, where these questions will be found fully discussed.

The evidence furnished by geology will not detain us long. It is distinct and unmistakable, and lends no countenance to any hypothesis of transmutation, development, or evolution. The intermediate forms which ought to connect the coincident species, according to the doctrine, are nowhere to be met with. If such ever existed, they must have been immensely numerous; and no reason is shewn, or can be conceived of, why they should not be preserved in a fossil state as well as other forms; yet are they nowhere found. On the contrary, species appear at once as true and perfect species, the incipient forms being nowhere met with, nor any such as to indicate that even what are called "extreme forms" had any stages of transition from others exhibiting a more simple structure. This is capable of detailed illustration from particular fossil species of the extreme or marked forms. Thus the huge marine saurian reptiles, or sea serpents of the primeval waters and great river mouths, have left their remains in almost every part of the world in the upper, secondary, and tertiary (?) strata. Vast numbers of individuals have been found; yet no incipient or transitional form has ever been met with. The same is true of the fossil bats, "which present the exact type of the existing forms" (*Mivart's Gen. of Sp.*, p. 130); of the fossil group of flying lizards, and of the *Chelonians* (turtles and tortoises),—all of them groups of very extreme forms. The *Monotremes* of Australia (duckbill platypus and echidna) are the lowest form of the mammalian type; the *Marsupials* (kangaroos and opposums), the next in order; the placental, or true young-suckling mammals,

* The argument has been long familiar to students of Ethics; it was well set forth by the learned Archbishop in popular lectures which were afterwards published.

the highest. But the order of their appearance in the strata is not the same as this. The marsupials appear first, and in the middle oolite, and are, according to Professor Huxley, just as perfect as existing marsupials; while the monotremes, the supposed link between reptiles and mammals, have not been found fossil. Mr. Darwin, however, feels sure that they will yet be found in a fossil state. As far, however, as evidence now goes, we must conclude that monotremes came into being after the more perfect mammals. In like manner, we do not find among the fossil fishes throughout the entire geological sequence any so low in the scale of organization as the existing *Pleuronectidæ* (flat fishes). The Ludlow beds of the upper Silurian formation first present us with very perfect examples of the vertebrate type in certain species of fishes, as highly organized as the higher forms of existing fishes. There are no transitional forms; no link connecting the molluscan type of structure with the vertebrate. The axial filament in the larva of the *Ascidian* (an organism, "hardly appearing like an animal," and certainly far below the lowest mollusc) though said to resemble the *Chorda dorsalis* of the vertebrates, cannot be regarded as a link between the two structures. Thus the challenge of Cuvier to Geoffroy St. Hilaire has never been met. "Shew me," said he, "a form of transition from the mollusca to the vertebrata, and I will begin to consider your hypothesis of transmutation." In regard to what are described as the connecting links between birds and reptiles (*Desc. of Man*, i., 204), there are anatomical difficulties into which I cannot here enter. They seem to point to the "sudden upstarting" of the same characters of skeleton and brain, and to a double relationship between the flying reptiles and common birds (those having a keeled breast bone) and the *Struthious* birds and *Dinosaurians* (Iguanodon and others). (See Mivart's *Gen. of Spec.*, pp. 70, 74, 131.) The believer in design and adaptation may well consider there would be close analogies of structure between the respective groups, two sets being aerial in their habits and two terrestrial only.—In regard now to the great class of mollusca, whose fossil remains far outnumber all the others, we have this curious fact, that the highest, the *Cephalopods*, and the lowest, the *Brachiopods*, appear nearly together in the fossil record. Both attain their maximum of development in palæozoic times, and both are now in the act of becoming extinct. While the second and third groups, the *Gasteropods* and *Acephala*, beginning about the same time, have grown in importance, and have their maximum in the existing creation

(Haughton's *Geology*, p. 346, 2nd ed.; Lyell's *Elements*, *passim*). The *Crustacea* had a much greater development, and in much larger individuals than the existing creation. Of this class are the *Trilobites*, which had their greatest development in the Silurian period, and became extinct in the Carboniferous.

Since I addressed you last year, the life horizon of this family has been carried down 4,000 feet. Species have been discovered at this depth below any bed in which they had before been found; a depth of deposit representing of course a vast lapse of time, and yet no difference in structure or characters is to be observed. Through the whole Silurian formation, indeed, which must represent a lapse of time inconceivably great, the structure of these creatures differs very little, and the oldest forms are as well furnished with eyes as the newest—an eye of a very singular and complex character. There are, indeed, variations in the organic characters; but this was to be expected in so vast a formation, representing many changes of external conditions; but the variations, as Mr. Gwyn Jeffreys has shewn, do not proceed from a single species or point, but from several points. The evidence from them is thus wholly against evolution. In regard to these creatures, it was discovered last year, for the first time, that they underwent metamorphoses like our existing crabs, and had eight pair of legs. By these discoveries the *Trilobites* are placed next to, if not actually with, the modern *Isopods*" (*Pres. Add. Geol. Soc.*, vol. xxvii., pt. ii., May, 1871). Like these recent *Isopods*, the *Trilobite* had the singular instinct of rolling itself up into a ball, and dropping down in case of danger. It may be stated, in conclusion, that the great mass of evidence drawn from palæontology is entirely opposed to the view of minute and gradual modification, and favours much more the sudden uprising of new forms, without any intervention of incipient stages.

I have alluded already to the permanency of species as marked by the infertility of hybrids. Geology furnishes many striking instances of this persistency of organic forms. I described last year the results of the late Atlantic dredgings. Here, at profound depths, many species were found living, which were only before known as fossil in the chalk and tertiaries, and were thought to be extinct. I instanced also Silurian species still living; and the singular case of the two reptiles of the Elgin new red sandstone, one exactly like a modern Gecko, the other in all respects like a New Zealand lizard (*Add.*, 1870, p. 24). A late statement by Professor Huxley bears strongly on this point. He says, "The significance of persistent

types, and the small amount of change which has taken place, become greater and greater in my eyes the longer I occupy myself with the biology of the past. . . . It is certain that a highly organized vertebrate type is capable of persisting with no considerable change through the period represented by the Carboniferous, Permian, and Triassic formations" (*Jour. Geol. Soc.*, May, 1870). Though a friend to the hypothesis of evolution, he admits that the whole invertebrate fauna and the lower groups of the vertebrata furnish no evidence in its favour. Such evidence, however, he considers he has found in the higher quadrupeds of the tertiary strata. This instance is that of the *Hipparion* and other forms allied to the existing horse—forms which differ more, *inter se*, than the horse, zebra, and ass, which are true and perfect species. Unable to handle the anatomical question, I attempted in my address last year to set aside the argument for evolution by two remarks. First, that if evolution was the Law of Life, it was strange that the vast suite of formations and all the varied forms of life entombed in them should furnish, on Professor Huxley's own shewing, no evidence in its favour, and so many cases of the permanency of organic forms. And, secondly, that granting a modification of form within certain limits, related to the constitution and vital force of each particular species, it was reasonable to suppose that a relationship to the wants of man, soon to appear upon the scene, known to the mind of the Creator guided these modifications in a direction to harmonize the structure with the foreseen conditions (*Add.*, 1870, p. 27). I was pleased, therefore, to find that Mr. Mivart, who is an accomplished anatomist, had made the following observations on this subject in his *Genesis of Species*, published in January last:—"In the case of the *Hipparion* and related forms, there is no proof whatever of modification by minute steps,—*a fortiori*, no proof of modification by 'Natural Selection' acting upon fortuitous variations. On the contrary, the series is an admirable example of successive modifications in one special direction along one beneficial line; and the advocate for design must be allowed to consider that one motive for this modification (among many inconceivable by us) was the relationship in which the horse was to stand to the human inhabitants of this planet" (*Gen. of Spec.*, p. 133).

The paucity of life in the Silurian and other older stratified deposits is admitted by Mr. Darwin to be inexplicable, and a solid argument against his views, according to which life should be abundant and varied. At the same time there are so many remains of animals

of high organization, and made on so many different patterns, with complex and perfect eyes and other organs of sense complete, in forms of life quite distinct, that already evolution must have done its work, and little more remained to be done. Now, the beginning of the Silurian period is inconceivably remote; what date of remoteness, then, must the beginning of those evolutionary processes have which brought into being all the wondrous forms of the Silurian formation? If it be a moderate computation to allow twenty-five millions of years for the deposition of all the strata to the top of the Silurian system, this can hardly represent the hundredth part of the time required for the evolution of the animal kingdom to its present state (Mivart, *Gen. of Species*, p. 136). This will make the whole period two thousand five hundred millions of years. Now, from three independent lines of enquiry, Sir William Thomson has shewn that the earth has been fit for life during about one hundred millions of years only, or one twenty-fifth part of the time which the other theory requires. These lines of enquiry are—(1.) The greater velocity of rotation of the earth in time past, deduced from the now discovered secular retardation due to tidal friction; (2.) the present state of underground temperature and known conductivity of rocks, indicating to us what the surface temperature must have been at a given epoch in time past; and (3.) the dynamical theory of the sun's heat and dissipation of energy rendering it impossible that the sun can have shone upon the earth for more than a certain length of time. But there is necessarily much uncertainty in all such estimates; and in regard to Sir William's reasoning, though it has not been seriously questioned by any one, it may be true within wide limits only. Still it stands as a most telling objection to the hypothesis, on the score of time simply.

Many persons who have given considerable thought to this subject and rejected Darwin's view of evolution, hold not the less a modified view of the doctrine as it relates to animals. They consider successive creations by an Almighty fiat as unnecessary, and reason that as laws of affinity and force have been impressed upon inorganic matter which, by their simple operation have produced all the crystalline minerals, are still occasionally producing a few in nature, and, under man's control, multitudes in processes of art, so an innate power, force, or tendency is implanted in that which has life, in virtue of which new forms are produced and harmonious results worked out under a Presiding Intelligence or Creative Will. This they term derivate creation, *i. e.*, divine

action by and through natural laws. They look upon this as a higher view of the Divine character, considering that a continual interference suggests a limitation of the Creator's power and an introduction of the supernatural into the domain of science. "Thus an internal law presides over the actions of every part of every individual, of every organism as a unit, and over the organic world as a whole. Such internal innate force causes the manifestation of new species by ordinary generation, the new forms being not monstrosities but harmonious self-consistent wholes, specific distinctness arising out of obscure sexual modifications. The stimulus of external conditions determines these sudden starts which are considerable in comparison with the minute variations of Natural Selection,—are in fact sensible steps, such as discriminate species; while the external influences are the result of an harmonious action underlying the whole of nature, cosmical, vital, and social" (Mivart, *Gen. of Species*, p. 239). Such a creation by Law, as opposed to absolute creation, was a necessary conception for those who could not believe Organization to be Life, and yet were not prepared to regard the origin of species as arising from a constant succession of absolute or distinct acts of creation, which they do not admit as being supernatural. This is not the place to enter further into such a subject, and I observe in regard to it this only, that it seems to me to put the Divine Being too far away from us—to make him less a personal God than He is represented to us in the Scriptures; and that it does not seem to me to provide for the Advent of man in the double physical structure, for his sudden appearance on the other side of the "enormous gulf," with the great mental powers necessary for the embodiment of thought in contrivances to protect his bodily frame—the most feeble and helpless among animals.

As regards the supernatural, it is in vain that we try to escape from it. We may roll it back as far as possible, but it will come upon us at last, and we must grapple with the difficulty.

There are three principal conceptions in regard to this subject:—

1. *That matter is eternal and self-existent.*—Against this the modern progress of science speaks loudly; and the notion has now very few supporters.

2. *Spontaneous generation.*—Of this Professor Huxley, in his address to the British Association at Liverpool, disposed so completely, that it must be abandoned by most persons.

3. *That organization is life—that it produces life.*—But the highest authorities on these subjects will not now admit this. They recognize life as a real active force which builds up the

bodily organs, though they do not propose to explain its nature. Professor Tyndal observes in regard to the notion, that if we knew that all our thoughts and sentiments were connected with molecular movements in the brain—if, for example, we knew that a right-handed motion of certain molecules round a line or point were connected with *love*, and a left-handed motion with *hate*—we should be no nearer than before to the solution of the difficulty. Besides this, all the intuitions of mankind, in regard to an immortal life, run counter to the notion.

We are thus thrown back upon the agency of creative power for the first origin of life—a supernatural act, of which science can take no account, of which she can offer no explanation. "She may change the venue"—she may relegate it to a different sphere—but this is only shifting the difficulty one step further back.

And here, Gentlemen, I am naturally led to offer some remarks on a suggestion lately thrown out by Sir William Thomson (*Ad. Br. Assoc.*, 1871), which seems to me to present a difficulty of this character. It was, as you will remember, to the effect that seed-bearing meteoric stones may have brought to our earth the germs of vegetation. Animal ova are not directly mentioned; but, from some collateral allusions, I suppose they are meant also. This is, indeed, almost certain from the following sentence:—"From the earth, stocked with such vegetation as it could receive meteorically, teeming with all the endless varieties of plants and animals which now inhabit it, the step is prodigious; yet, according to the principle of continuity, most ably laid before the Association by a predecessor in this chair (Mr. Grove), all creatures now living on earth have proceeded by orderly evolution from some such origin." Such a hypothetical origin for life on the earth has startled not a few persons almost "out of the proprieties." One able writer, an active member of the Astronomical Society, regards the whole thing as a jest on Sir William's part, and makes merry at the expense of those who regard Sir William as being in earnest, suggesting that the Scotch may now retort on the English the gibe of Sydney Smith about their slow apprehension of humour (*St. Paul's Mag.*, Sept., 1871). I believe, however, that this is an entire misconception, and that Sir William was quite in earnest. It would have been a manifestation of bad taste, of which Sir William is wholly incapable, to jest on such an occasion and such a subject. It was meant in good faith; and, though confessedly wild, improbable, and startling, was not unscientific. We know that there are vast numbers of meteorites moving

about in all directions; and not only so, but combined flights of meteorites one hundred thousand miles wide and a thousand millions of miles long are crossed by the earth every year. The quantity of diffused matter of this kind must be immense; and comets may be but the gathered-up materials of meteors, or groups of meteors, made luminous by collisions among the component small masses, as when a horse's shoe strikes fire with the pavement. It is, again, within the range of probability that collisions of even larger bodies may take place in space, but can by no means be so likely by thousands or millions of times to happen as of ships in the Atlantic; and as to broken-off fragments of distant bodies reaching our earth, you will remember the striking case I brought before you two years ago, when submitting to you an estimate of the labours of the late Thomas Graham. He analyzed a meteorite, and found it to contain three times as much hydrogen as can be forced into malleable iron under atmospheric pressure; hence the metallic mass must have been burnt in a dense hydrogen atmosphere, whence it was driven off, and for which we must look beyond the cometary matter within the Solar System. It must have come from the far-off regions of interstellar space, bearing to us the imprisoned hydrogen of the stars! Such a meteorite could not be expected to bear with it any seed-germs or animal ova from the burning star or sun whence it came; and why may not many of them have such an origin? It was once held highly probable that these strange bodies were fragments of a large planet, lying between Mars and Jupiter, which burst under some wondrous explosive force, and sent its fragments off widely through space; and that the one hundred and seventeen small planets which circulate there were some of the larger fragments, still continuing to obey the sun's force as their centres of movement, others being projected far out into space, so as often to impinge upon our earth. Now if the asteroids which actually circulate round the sun had been thus produced, they would all return to the point where the explosion took place; but this is found to happen in the case of two only—Pallas and Juno—so that we can no longer consider such an origin possible for them. Of explosions and collisions we in truth know nothing; and there is only one case of a burnt star, *t Coronæ*, burnt down in twelve days from the second to the eighth magnitude, probably from the exhaustion of a vast hydrogen envelope, as the flame shewed in the Spectroscope the hydrogen lines.

But suppose, now, that meteorites moving through space have

not had any such fiery origin as those to which I have referred, how are they circumstanced? They are exposed to all the cold of the interstellar spaces, which is reckoned at about 240° F. below zero. To such an utter negation of all heat would the seed-germs and ova be exposed for many years, if they came from the region of the stars, and shorter in proportion, if from bodies nearer, say from some habitable globe among the planets? Now, we know that seeds germinate after being frozen up for months, and after two or three thousand years of repose among the mummy dust; but we can hardly suppose that any seed or ovum would retain its vitality after exposure for even a short time to such a degree of cold as 240° F. below zero. Then we know what the result must be, when such meteorite enters the resisting medium of our atmosphere fifty to one hundred miles above the earth's surface. By the condensation of the air in front of it in its rapid flight, such a heat is soon developed from the air, that the whole mass is ignited, exploded, and scattered into fragments. It is conceivable, however, that splinters may escape ignition; or that the instant the resisting medium is entered, and before the heat is yet developed, particles on the surface may be whisked off and thus escape ignition. Among this dust there may be germs and ova, which, though long floating far aloft, and contributing to the colour of the sky by their polarizing effects upon light, agreeably to Tyndal's discoveries, may at last be dashed to the ground on descending currents and find a lodgment in the soil.

But even if we allow such possibilities, are we any nearer the solution of the origin of life? What was its origin in the habitable world whence the meteorite is supposed to have been launched? It came there by a meteoric messenger from a pre-existing source of life, and so on in endless succession, till at last we are obliged to rest in a creative act for an origin of Life. And in this it would have been as well to take refuge at the first.

But there is a further objection. The vital power of a species, in either kingdom, and its implanted tendency to development, must have relation to the conditions in which it is placed, even in the embryo state, else its creation is a failure; created for conditions in one habitable globe, it would not be adapted for development in another unless the conditions there were very similar. Now, from all we know of habitable globes in the Solar System, we are sure the conditions in question are very different; the speculation must therefore be laden with another most singular and critical condition, that the world whence the meteorite was

shot forth was very like our own. And lastly, suppose even we were to adopt as an article of scientific faith Mr. Grove's "doctrine of continuity," which cannot be admitted till species are proved to coincide by variation, where is the origin for man? Has he, too, "with all creatures now living on the earth, been produced by orderly evolution" from one of these ova germs?*

Though regretting that Sir William has by these statements apparently given countenance to the hypothesis of evolution, I rejoice that he has commended the study of the long-neglected subject of Teleology. He has called attention to the argument for design or foreseen use from the animal and vegetable structures, and cosmical arrangements in general, which for many years have been but little put forward, perhaps partly from the causes he has mentioned, and partly, in consequence of the views which have long held sway in regard to the origin of life.† I would earnestly urge this line of thought and of study on the younger members of the Society; and I do so the more earnestly that I may not have another opportunity, as I now demit my office of President. And I would ask you further to remember, that what is called scientific evidence

* The terms in which Sir William Thomson here refers to the "doctrine of continuity," doubtless from the brevity unavoidable at the close of a long discourse, would lead one who had not read Mr. Grove's address to suppose that he had arrived at very positive conclusions in regard to "orderly evolution," and by a line of argument different from that of Mr. Darwin. Such, however, is not the case; on the contrary, he distinctly disclaims (*Brit. Ass. Rep.*, 1866, p. lxx.) any such attempt, in so far as the history of organic beings is concerned. He merely reproduces arguments of Mr. Darwin and other advocates of evolution; stating them, however, in a manner much less dogmatic than Mr. Darwin. In other portions of the address Mr. Grove merely illustrates the intimate relation of the forces of nature—a subject on which he has produced a most able treatise—the position that the connection of all natural phenomena is more clearly seen as discovery advances, and the gradual progress of mankind in their social institutions and their appreciation of Liberty and Law. Many objections might be urged against his views in as far as they relate to the greatest efforts of the human intellect in the walks of science, literature, and art, and even in control over the minds of men and the events of history; but criticisms of this kind have no relation to our present subject. See note, p. 24.

† Feeling much uncertainty as to the causes of the neglect referred to in the text, and doubting that assigned by Sir W. Thomson, I took the liberty of requesting Professor Calderwood's opinion in regard to it; and I have his kind permission to publish his reply:—"Edinburgh, October 31, 1871. . . . That the argument for design has been less used is, I think, true. The explanation, I believe, to be partly physical and partly metaphysical. (1.) Physical Science, under the impulse of immense advancement, hoped to find in a development theory an explanation of all existence—the origin of organic *from* inorganic matter. Failure has come, so that now Professor Huxley

is not a whit better than evidence of another kind, the Evidence of Testimony, on which we receive the truth of certain events and transactions which have taken place among men. Unfortunately, by the very constitution of their minds "men who are constantly exercised in observing by the senses or, in processes of inductive and deductive reasoning about objects of sense, receive vivid impressions from this kind of truth only, and do not properly estimate other kinds of evidence. Thus a man who can estimate a mathematical demonstration, or one who can found sound inductive reasoning on his observations, may be incapable of feeling the conclusiveness of the most perfect moral or historical argument; and such persons will tell us that mental and moral science is unsatisfactory, because its phenomena are not manifested to the senses; they are, in fact, sceptical regarding truths which rest *only* (!) on the evidence of testimony."* Results deduced by proofs of this kind are just as certain and true as those which are usually called scientific. Deduced in a different way, and having regard to matters wholly diverse in their nature, they may not harmonize—they may even seem often to be in most serious conflict. Yet, do not dread science; but be slow to embrace hasty conclusions: to such young inquirers are too apt to rush, especially if hypotheses or theories have a semblance of completeness, if they pretend to explain all physical or vital phenomena. How many such have had their day, and are now consigned to oblivion! Have confidence in science; she will yet right herself, and by her own methods; already she has done so to a large extent in this century, as I have before observed in regard to materialistic views and the origin of life—and she is still progressing along the same hopeful path. Rest assured that if we can only wait patiently and maintain serene tempers, all will come right in the long run; truth will harmonize with truth, and there will be but one universal conviction in regard to the great subjects of belief which most nearly concern us all.

has abandoned the attempt, and has come round to a spiritualistic philosophy; and that means the existence of the Great Spirit. (2.) Metaphysical Science, advanced by the investigations of Kant, has parted from the old logical processes on which to found. Kant has shewn that no argument in proof of the Divine existence is competent. This has partly contributed to the result to which you point. But a theory of the universe, which does not make Mind its final explanation, seems to me self-condemned. The theorist is above his theory."

* *Lectures to Young Men*, Glasgow, 1849: "Moral Influence of Modern Prose Literature," by the Rev. Dr. Bryce.

In retiring from this Chair I have the satisfaction of knowing that the Society is in a more flourishing condition than at any former period of its history; and that the roll, which has been nearly doubled in the last three years, contains the names of so many Members engaged in original research, that the high character of the Society will be not only maintained but advanced in years to come. Nearly all the scientific Societies of the city have become affiliated with our Society as sections; and all that is now wanted for the complete development of the advantages thus secured is the possession of a building, the sole property of the Society, which should contain more suitable and ample accommodation than we now have for our various Meetings, and for the extension and proper display of our library. Let me express the hope that the earnest efforts of the Society will now be directed to the accomplishment of this object.

In conclusion, Gentlemen, I have to thank you for the patient attention you have given me on this occasion, for the uniform support I have received from you, and the kind indulgence you have extended towards me, in my feeble endeavours to discharge adequately the duties of the important office to which I was raised by your kindness.

INAUGURAL ADDRESS

OF

PROFESSOR GRANT, M.A., LL.D., F.R.S.,

THE PRESIDENT-ELECT.

THE ASTRONOMY OF THE NINETEENTH CENTURY.

ON this occasion, when I have the honour of addressing you for the first time as President of this Society, I would beg to make a few remarks on the present state of astronomical science, with especial reference to the discoveries and improvements achieved by astronomers since the beginning of the nineteenth century. In a field so vast, embracing so many subjects of deep and varied interest, it will manifestly be impossible for me to enter into much detail. I shall therefore confine myself to presenting you with an outline of some of the more important accessions which the different branches of the science have received during the period under consideration. But before entering upon this task, it may be desirable to survey for a moment the state of astronomy as it existed at the beginning of the nineteenth century.

Commencing with physical astronomy—unquestionably the most sublime branch of this great science—we recognise the marvellous success with which the theory of gravitation, as propounded by Newton, had been developed by the labours of Euler, Clairaut, D'Alembert, Lagrange, and Laplace. The disturbing effects produced by the mutual attraction of the various bodies of the planetary system had formed the subject of persevering investigation. The inconceivably slow variations of the elements of the planetary orbits, arising from this cause, had been discovered by Lagrange to be characterised by recurring cycles of values oscillating within narrow limits. In other words, the stability of the solar system as affected by the disturbing action of the planets upon each other was assured throughout an indefinite succession of future ages. Equally interesting were the results arrived at relative to the irregularities of shorter duration in the movements

of the planets, which have been denominated the periodic inequalities. Analytical expressions were obtained for each planet, involving the effects produced by the disturbing action of the other planets of the system, by which it was possible to compute the exact place of the planet in the heavens for any assigned time, whether past or future. The great inequality in the movements of Jupiter and Saturn, which had acquired a high historic interest from the labours of astronomers in successive ages, and which had so much perplexed the illustrious successors of Newton, was finally traced to its true physical source by the sagacity of Laplace. The secondary planets of the system had formed the basis of much admirable research. The complex irregularities in the motion of the moon, depending on the disturbing force of the sun, had been analysed and traced to their respective sources with consummate skill; and the practical application of the theory of the movement of the earth's satellite to the great problem of the longitude had finally become an accomplished fact. The theory of the system of Jupiter's satellites had been fully investigated by Lagrange and Laplace, and supplied a new and interesting body of facts in support of the great law of gravitation. The difficulties peculiarly affecting the theory of the movements of comets had been encountered and overcome, as was triumphantly evinced by Clairaut's computation of the return to the perihelion of Halley's comet in 1759, and by the subsequent researches of Lagrange. Similarly brilliant results crowned the researches of the continental geometers on the figure of the earth and the other great problems of the theory of gravitation. Finally, Laplace had undertaken to present to the world in one great work a monumental exposition of the entire subject of celestial mechanics.

With respect to the physics of the planetary system, the progress of researches at the time to which I refer had been inconsiderable. Telescopes of unprecedented power had, however, been recently applied by the elder Herschel to the scrutiny of the various bodies of the solar system, and some interesting results were arrived at, which I shall have occasion hereafter to mention. Furthermore, the same astronomer had discovered a new planet of vast dimensions revolving round the sun beyond the orbit of Saturn.

Stellar astronomy, which has been characterised by so many brilliant researches in recent years, was acquiring form and substance from the immortal researches of its great founder, William Herschel, who was then in the middle of his career. It will be desirable, however, to postpone any special allusion to those re-

searches until we have occasion to refer to the more recent labours in this branch of astronomy. I now proceed to the subject proper of this address.

The theory of the movement of the earth, considered as one of the bodies of the planetary system, occupies its share of the astronomer's attention. But independently of this circumstance the subject is entitled on other grounds to a most searching investigation. This will appear obvious when it is considered that the observations of the celestial bodies are necessarily made from positions on the earth's surface, and that consequently the acquisition of an accurate knowledge of the motion of the earth round the sun becomes an object of fundamental importance. Once in possession of this knowledge, the astronomer is enabled to transfer his observations from the earth to the sun, the true physical centre of the planetary system. On the other hand, if there should be any error in the theory of the earth's motion, the observed places of the planets would all be vitiated from this cause in the process of reduction to the sun, and the effect of such an error would thus extend over the whole planetary system. An irregularity in the earth's motion, which in the present century gave much trouble to astronomers was finally traced to its true source by Airy, who discovered its origin in the disturbing action of the planet Venus. It arises from the circumstance that thirteen revolutions of the planet are very nearly equal in respect of time to eight revolutions of the earth. In consequence of this very close coincidence, the effects of the disturbing attraction of the planet, although small essentially, go on for many years accumulating in the same direction until they ultimately attain an aggregate magnitude of sensible value. A process the reverse of this then occurs, the inequality gradually diminishing in the same manner as it had previously increased, and finally completing the cycle of its values in a period of 240 years. The discovery of this inequality, small as it is (at its maximum it amounts to only 3"), introduced a marked improvement into the solar tables.

The planetary system may be supposed to be divided into three groups of bodies, the interior group, the intermediate, and the exterior group. The interior group comprises the planets Mercury, Venus, the Earth, and Mars. The swarm of minor planets revolving between Mars and Jupiter constitute the intermediate group. The exterior group includes the planets Jupiter, Saturn, Uranus, and Neptune. The theory of the movements of the interior planets has formed the subject of a most searching investigation by Le Verrier, who has constructed, upon the basis of his researches,

tables of the movement of each planet, the results of which represent the observed places of the planet with a degree of accordance far closer than had been previously arrived at.

The discovery of the group of minor planets revolving between Mars and Jupiter and the investigation of the theory of their movements, constitute one of the most interesting subjects of the astronomy of the nineteenth century. The first of this singular group, the planet Ceres, was discovered on the first day of the year 1800, by the Italian astronomer Piazzi, and this achievement was followed in a few years afterwards by the discovery of three additional bodies of the same group. These four planets, Ceres, Pallas, Juno, and Vesta constituted for many years the only members of the group known to exist. At length in 1845 a fifth planet of the group was discovered by Encke, a German astronomer who had for many years been occupied in searching for additional planets in the same region. It seemed now to be firmly established in the minds of astronomers that there existed many more undiscovered members of the group, and a diligent search was commenced by various observers in search of new planets. These efforts have been attended with complete success. Every year which has elapsed since 1845 has been signalised by fresh discoveries in this field of observation, insomuch that in the present day the number of planets of the group amounts to one hundred and seventeen. Hind, Goldschmidt, Luther, Chacornac, De Gasparis, and Watson have especially distinguished themselves in this field of research.

The investigation of the theory of the movements of the minor planets is due almost entirely to the German astronomers. The first difficulty which presents itself upon the discovery of a new planet is the determination of the elements of its orbit. In so far as regards the ancient planets, the elements of their orbits were originally determined by the Greek astronomers, and in the lapse of ages the results received a gradual succession of improvements so that in the present day any new determination of the elements would consist simply in applying a correction to the elements already in use. It is quite otherwise, however, when the question refers to a new planet. In this case the astronomer has no previous orbit to guide him in his inquiry, and he must devise a special method of computation adapted to the exigencies of the occasion. The problem may be thus proposed: from a definite number of observed places of the planet to calculate the elliptic elements of its orbit. The corresponding problem for a body revolving in a parabola had been already solved by Newton, and in the eighteenth century the problem for the ellipse had been

attacked by various mathematicians with more or less success. To Gauss astronomy owes a method of computing the elliptic orbit of a body revolving round the sun from three observations, which has been of inestimable value in establishing the theory of the movements of the minor planets.

Another difficulty offers itself in the investigation of the movements of this group of bodies. The computation of the disturbing action which one planet exercises over another is immensely facilitated in the case of the principal planets by the circumstance that their orbits are only slightly excentric, and that the planes in which they revolve are inclined to each other at comparatively inconsiderable angles. But with respect to the group of minor planets the case is altogether different, the orbits of those bodies being very excentric, and their planes being inclined at considerable angles to the planes of the orbits in which the principal disturbing bodies revolve. This difficulty had already occurred in the theory of comets, and a special method, called the method of quadratures, had to be devised for computing the motion of the disturbed body. This method, however, was not based upon the mean elements of the disturbing and disturbed bodies, and the resulting formulæ were not applicable as in the case of the principal planets of the system to the computation of the place of the planet in the heavens for any time, whether past or future. Thanks to the researches of Encke, Hansen, and Brünnow, the computation of the disturbing effects produced in the movements of the minor planets no longer offers any serious difficulty, and the places of those insignificant bodies which, in many instances, require a very powerful telescope to reveal their existence may now be traced out in the heavens with as great facility as the places of Uranus or Neptune. The apparent diameters have no appreciable value. It is impossible, therefore, to ascertain the absolute magnitude of any of those bodies in the way usually employed by astronomers. Attempts, however, have been made to effect this object by photometric experiments on the light emitted by the different bodies. In this way some very curious results have been arrived at. The absolute diameter of the largest of the group, the planet Vesta, measures only 228 miles. The diameters of the other bodies are in general much smaller. Euterpe, for instance, has a diameter of 39 miles, Pomona 35 miles, and so with respect to the others.

In all probability the number of the minor planets amounts to thousands or even millions. It is certain, however, that their aggregate mass is inconsiderable. Le Verrier has found that if it

amounted to one-fourth of the earth's mass, the group of bodies could not fail to exercise a sensible disturbing influence on the other bodies of the planetary system. No such effect, however, has been recognised; and we may therefore conclude that the aggregate mass of the group does not amount to a millionth part of the sun's mass.

Two important results have been arrived at in the present century relative to the exterior group of planets. One of these refers to the planet Jupiter. From its position in the solar system and its great mass, this planet exercises a powerful disturbing influence on the various planets and comets revolving round the sun. It is, therefore, an object of fundamental importance that a correct determination of the mass of the planet should be available to the astronomer, so that he may be enabled to calculate with a corresponding degree of accuracy the effect produced in all cases by its disturbing action.

The mass of Jupiter was originally derived by Newton from the greatest elongations of the fourth satellite, as measured by Pound, a contemporary astronomer. This result continued to be adopted by astronomers till the year 1830, when Airy, having re-measured the elongations of the satellite at Cambridge, discovered that an error of sensible magnitude had been committed by Pound, and that in consequence thereof the adopted value of the planet's mass should be considerably increased. Nearly about the same time, and quite independently of Airy, it was discovered by Encke that a corresponding increase in the adopted value of the planet's mass was necessary, in order to account for the observed irregularities in the elliptic movement of the comet which bears his name. Nicolai also, another German astronomer, derived a similar result from an investigation of the irregularities in the elliptic movement of the minor planet Juno. The results accruing from these three independent sources agreed all very nearly with each other, and have led to the adoption of an improved value of the mass of the principal planet of the solar system, which has exercised an important influence on the progress of modern astronomy.

The other circumstance connected with the exterior group of planets, to which I would allude, has reference to the discovery of the planet Neptune. This may be regarded as one of the most brilliant achievements recorded in the annals of science. As you are all no doubt aware, it originated in the efforts made by astronomers to account for certain unexplained irregularities in the movement of the planet Uranus. This planet had been discovered by William Herschel in 1781; but some interval of time was required to elapse

before a sufficient number of observations of its place in the heavens could be obtained, which would form a sure groundwork for computing the elements of its orbit. Finally, however, in 1821, an orbit of the planet was computed by the French astronomer Bouvard, and tables of the planet were constructed upon its basis.

For a little while the movement of the planet seemed to go on all right, but soon the wanderer fell out of its place, and the discordance between the apparent place, as determined by observation, and the place calculated from the tables, increased from year to year. Now Bouvard, in his theory of the movement of the planet which had formed the basis of the calculated places, had strictly taken into account the effects produced by the disturbing action of all the known planets of the system. The question which now, therefore, offered itself was this, To what cause are the outstanding irregularities in the movement of the planet attributable? I need not state here how the question was taken up by Adams at Cambridge and Le Verrier in Paris, quite independently of each other, and how they both solved the problem of the irregularities of the planet, by attributing them to the disturbing influence of a hitherto unknown exterior planet, to which they assigned a position in the heavens, agreeing almost exactly with each other. The results of Le Verrier's calculations were received at the Berlin Observatory on the 23d of September, 1846. On the same evening the heavens were explored, and the planet was discovered within a degree of the place assigned by Le Verrier's calculations. A search for the planet had been instituted at Cambridge by Professor Challis, and two positions were obtained of it as a star, which, however, when they came afterwards to be sifted, would infallibly have resulted in the discovery of the planet. This great discovery, with which the names of Adams and Le Verrier will be immortally associated, is one of the most brilliant triumphs which has yet been achieved in the development of the theory of gravitation.

An accurate determination of the magnitude and figure of the earth is an object of fundamental importance in astronomy. This will be readily understood when we reflect on the circumstance that the radius of the earth forms the base line of all the measurements relating to the distances and magnitudes of the celestial bodies. The spherical form of the earth is a fact so obvious that it was recognised at a comparatively early period of Grecian civilisation. It is otherwise, however, when the question refers to the ellipticity. Newton first remarked, as one of the consequences of his great discovery of the law of gravitation, that the mutual attraction

of the particles of the earth's mass, combined with its rotation on its axis, would cause it to bulge out at the equator, and to be flattened at the poles. It is well known to all who are acquainted with the history of astronomy that this sagacious inference was verified in the following century by a comparison of arcs of the meridian, measured in Peru and Lapland under the auspices of the French Academy of Sciences. The elements of the earth's figure, as determined in recent times, are based chiefly upon four great arcs of the meridian. These are:—the French arc of the meridian, extending from Dunkirk to Formentera, one of the Balearic Isles, embracing an amplitude of 14° 15′; the British arc, extending from Dunnose, in the Isle of Wight, to Saxavord in the Shetland Isles, embracing an amplitude of 10° 7′; the Russo-Scandinavian arc, extending from Ismail on the Danube, to Fuglænos in Norway, embracing an amplitude of 25° 20′; and the Indian arc, extending from the base of the Himalaya mountains to Cape Comorin, embracing an amplitude of 21° 30′. Besides these colossal arcs, smaller arcs of the meridian, and also parallels of latitude, have been measured in various parts of the world; and upon the basis of such materials the dimensions of the equatorial and polar axes of the earth have been independently determined by Airy and Bessel, and still more recently by Clarke; and the results obtained by these investigators exhibit a remarkable accordance with each other. The theory of gravitation furnishes two striking confirmations of this result, derived from two independent sources.

One of these methods consists in swinging the pendulum in different latitudes. It is easy to perceive that if the earth be flattened at the poles, the force of gravity soliciting an object to the earth must be less intense at the equator than at the poles; for at the equator a stone, for example, besides being further removed from the earth's centre, is under the influence of the centrifugal force generated by the diurnal rotation.

For the same cause, the intensity of gravity will vary at all intermediate distances between the equator and the poles. This variation of gravity will be different for different ellipticities; consequently, if we determine the variation of gravity by actual observation, we obtain an indication of the ellipticity.

Now, the pendulum puts us in possession of a simple and effective means of measuring the intensity of gravity. The greater the intensity of gravity, the greater will be the number of oscillations the pendulum will make in a given time—say twenty-four hours. Con-

sequently, by swinging the pendulum in different parts of the world, and counting the number of oscillations which it will make in a given time, we are enabled, taking advantage of a theorem due to Clairaut, to determine the ellipticity of the earth.

The pendulum has been swung in this manner in various parts of the world—by Biot, Sabine, Freycinet, Foster, and others—and the results assign to the ellipticity a value agreeing almost exactly with that derived from the measurement of arcs of the meridian. In fact, the pendulum makes the ellipticity to be $\frac{1}{289}$.

The third method which I may mention as having been employed for determining the ellipticity of the earth is still more extraordinary than the one to which I have just referred. It is founded on the circumstance that the earth does not attract the moon in the same manner as if it were a perfectly spherical body. Certain irregularities are consequently produced in the moon's motion, arising from the accumulation of matter at the equatorial regions of the earth. Now, just in the same manner as the pendulum will oscillate with greater or less rapidity according to the intensity of the force acting upon it, so the moon will be disturbed in her movement according as the earth is more or less elliptical. In this way the ellipticity of the earth was determined, and found to be $\frac{1}{304}$.

The mean density of the earth supplies another interesting question of astronomy, which has received several solutions in recent times. It is no doubt well known to you all that this question was first solved, towards the close of the last century, by experiments on mountain attraction in Perthshire. More recently experiments were made by Baily, with a delicate apparatus suggested originally for the purpose by Michell, and practically applied towards the close of the last century by Cavendish. The apparatus consisted essentially of a deal rod suspended by a fine wire, and having two leaden balls about two inches in diameter attached to its extremities. Two leaden spheres, about eight inches in diameter, were then brought near the smaller ones on opposite sides of the rod, and their attraction, combined with the torsion of the wire, produced an oscillatory horizontal motion of the rod on each side of a position of equilibrium. In this way Baily found the density to be 5·6. Cavendish originally found, by experiments of the same kind, 5·4 as the mean density. And it is curious that exactly the same result was subsequently obtained by Reich, a German physicist, who repeated the experiment.

Another method for effecting the object is due to Airy. It con-

sists in swinging a pendulum at the bottom of a deep coal mine, and also at the entrance into it. In this case the pendulum at the bottom of the mine will not be affected by the attraction of the shell of matter enclosing it; but will be attracted merely by the sphere upon which it rests. The two masses of matter may therefore be weighed against each other in the same way as a mountain is weighed against the earth's mass. The result in this case is not found to agree exactly with that derived from other sources. It was found to be 6·5. It is remarkable that Newton conjectured the density of the earth of be about five or six times the density of water.

There is no branch of astronomy upon which so much attention has been lavished as the theory of the movements of the moon. This is attributable to a combination of several circumstances. First, the inequalities in the moon's motion are of greater magnitude and of a more pronounced character than those relating to any other body of the planetary system. To unravel those numberless inequalities, to determine the laws by which they are governed, and to trace their physical causes, has been to the astronomer an object of earnest inquiry in all ages. Again, the moon being one of the two bodies upon which the phenomena of eclipses depend, a correct knowledge of her movements has been ardently desired by the astronomer, apart from the interest which the abstract beauty of the subject is calculated to inspire. Finally, when we consider that in modern times the most trustworthy solution of the great problem of finding the longitude at sea has been shewn to be derivable from an accurate theory of the moon's motion, we may readily account for the immense researches of which lunar astronomy has formed the groundwork.

The motion of the moon is the most ancient of the subjects of astronomy, reaching back as it does to the time when the Chaldeans observed eclipses from the towers of Babylon, about seven hundred years before the Christian era. The results of the Chaldean observations were employed by Hipparchus in establishing the theory of the moon's motion, and they have thus come to be engrafted on the long train of research, extending from the time of that astronomer down to the present day. Some of the more important of the inequalities which affect the elliptic motion of the moon were discovered by the Greek astronomers; several others were discovered by the Danish astronomer Tycho Brahé. It was reserved for Newton to shew that they all originated in the disturbing action of the sun. In the case of one

of the planets, we have to consider a body revolving round the sun as the great centre of attraction, and affected by certain irregularities in its motion, arising from the disturbing action of the other planets of the system. In the theory of the moon's motion, however, the central attracting body is the earth, and the disturbing bodies are the sun and the planets. Now, in consequence of his enormous mass the disturbing action of the sun vastly exceeds the combined action of the planets upon the moon or upon each other; and hence it arises that the irregularities affecting the elliptic motion of the moon are incomparably greater than the corresponding irregularities in the movements of any of the planets. In the eighteenth century, the analytical researches of Newton's successors led to an important extension of the theory of the moon's motion; and, finally, the lunar tables of Mayer were found to be practically useful for the purposes of navigation. The researches of Laplace, in the beginning of the present century, resulted in a further improvement of the lunar theory; and tables still more accurate than any that had preceded them were constructed by Bourg and Burckhardt. I have to remark, respecting the tables of any body of the planetary system, that the disturbances affecting the elliptic movement are represented by a series of inequalities of definite magnitude and definite duration, both depending in each case upon the elements of the disturbing and disturbed bodies. If, then, the astronomer has ascertained beforehand, by observation, the elements of the disturbing and disturbed bodies, he is in a position to calculate the various inequalities (whether elliptic or perturbative) which enter into the tables of the movement of any of the planetary bodies, whether primary or secondary, without any further appeal to observation. Hitherto, however, in the construction of lunar tables, the magnitude of the inequalities was in many instances determined directly from a comparison of the observed places of the moon, without any reference to the theoretical expressions of their respective values. Laplace, deeply impressed with the imperfection which attached to the lunar theory in this respect, induced the French Academy of Sciences, in the year 1818, to propose as a prize a theory of the moon's motion which should be dependent on observation only, for the six fundamental elements of the orbit.* Two memoirs were honoured conjointly with the

* The fundamental elements of the motion of a body revolving in an elliptic orbit are:—(1.) The mean distance or semi-major axis (which determines the mean motion or time of revolution); (2.) the excentricity; (3.) the longitude of

prize. One of these was due to Plana and Carlini, the other to Damoiseau. Tables were constructed upon the basis of Damoiseau's theory which were found to be quite as accurate as those of Burckhardt. In 1832, Plana published his theory of the moon, in which the approximations were pushed further than in Laplace's theory upon which the researches of Damoiseau were mainly based. Still, in all these efforts to solve the great problem of the moon's motion, the method of procedure was essentially the same in principle. In 1838, however, Hansen announced to the world a new method of solution, which he afterwards fully developed; and tables, constructed on the basis of this theory, were published in 1857 at the expense of the British Government. These tables were found to be vastly more accurate than any that preceded them. They have since been regularly employed in the computation of the moon's places, which are given in the Nautical Almanac from year to year.

I have now to refer to another great effort of intellectual research which the fascinating difficulties of the lunar theory have called forth in recent years. In 1846, Delaunay announced his discovery of a singularly original and beautiful method of solving the problem of the irregularities in the moon's motion. With a power of investigation and a perseverance rarely paralleled in the annals of science, he laboured for eighteen years in the development of this method. The details were finally given to the world, under the auspices of the French Academy of Sciences, in two magnificent volumes, one published in 1862, and the other in 1867. Another volume still remains for publication; but the main part of his immense task has been already executed by the author. It is understood that tables are in course of being constructed by Delaunay on the basis of his theory. The results cannot fail to be looked forward to with much interest by astronomers.*

the perihelion (or perigee in the case of the moon); (4.) the inclination of the orbit with respect to a fixed plane; (5.) the longitude of the ascending node; (6.) the mean longitude of the revolving body corresponding to a fixed epoch of time.

* In referring thus to the great work of Delaunay, it may not be out of place to quote the following extract from the address delivered by Professor Adams, on presenting to that astronomer the medal awarded to him by the Royal Astronomical Society, in consideration of the first two volumes of his work:—

"The present work is complete in itself; in it the very difficult and complicated problem of determining the moon's motion is attacked by a perfectly original method, and that one as powerful and beautiful as it is new. The work has been planned with admirable skill, and has been carried out with matchless

It remains for me to allude briefly to an important discovery in the lunar theory, which is due to Adams. One of the irregularities in the moon's motion, which occasioned most trouble to astronomers in the last century, consisted in the fact that the time of the moon's revolution round the earth was found to be gradually diminishing, from century to century, since the earliest ages of astronomical observation. The theory of gravitation seemed to be incapable of accounting for such a result. Still, the existence of a slow but steady acceleration of the moon's motion was undoubted, as Halley had shewn by a comparison of certain eclipses of the moon observed at Babylon about seven hundred years before the Christian era, with similar eclipses observed by the Arabians in the Middle Ages, and with the more recent observations of the moon's place as assigned by modern astronomers. The subject continued for a long time to perplex and baffle the efforts of astronomers, until Laplace finally pointed out its origin, and shewed it to be in perfect harmony with the principles of the theory of gravitation. The amount also which he assigned to the inequality appeared to agree closely with the magnitude derived from the records of astronomical observation. Such was the condition of the question, until Adams, having been led to investigate the subject, discovered that the researches of Laplace were imperfect, in so far as they were not carried to a sufficient degree of approximation; and that when this was effected, the resulting magnitude which Laplace had assigned to the inequality was diminished by about one-half. The result of Adams was keenly contested by several astronomers; but it was supported by the contemporaneous researches of Delaunay, Cayley, Donkin, and several others; and in the present day it is universally acknowledged to be correct.

The question which next arises is, since the magnitude of the inequality, as computed by Adams, amounts to only half the value assigned by observation, how are we to account for the other half of the observed value? It has been endeavoured to get over this difficulty by supposing a retardation of the earth's diurnal motion, due to the influence of the tides. The subject, however, has not yet been satisfactorily investigated.

The subject of comets is unquestionably one of the most interesting in the whole range of astronomy. The sudden manner in which

perseverance. The result is an enduring scientific monument, of which our age may well be proud, and which we are happy to distinguish, on this occasion of our fiftieth anniversary, with the highest marks of our approval which it is in our power to bestow."

bodies of this class generally present themselves to observation, the strange aspect by which they are distinguished, and the short period during which they continue to be visible, have the effect in every age of securing for them universal attention.

One of the most difficult problems which occurred to Newton, while engaged in the development of the theory of gravitation—indeed, to use his own words, by far the most difficult*—was presented by the movements of comets. It consisted in determining, from a definite number of observed places of the comet, the elements of the orbit in which it revolved. In this, as in other instances, his genius was triumphant. His solution of the problem, however, was founded upon the supposition that a comet while revolving in the neighbourhood of the sun describes a parabola, an assumption which leads to no sensible error, when the question refers to comets revolving in very excentric ellipses. He remarked that, while the five elements of the comet's orbit might be obtained by the parabolic hypothesis, the sixth element, or the time of revolution, might be ascertained by comparing together the elements of comets which appeared at different times. Halley's grand discovery of the periodicity of the comet which bears his name was the result of this happy suggestion of Newton's. Still, it might be that comets revolved in ellipses which were not so excentric as to coincide sensibly with a parabola near the perihelion. I have already referred to the important researches of Gauss on the orbits of bodies revolving in conic sections. By the application of the method which he invented the computation of the orbit of a comet no longer offers any serious difficulty. For determining the orbit of a comet revolving in a parabola, a simple method was devised, towards the close of the last century, by Olbers, a German astronomer. Curious enough, a method substantially the same was several years afterwards re-invented by our countryman, Ivory, and published in the *Transactions* of the Royal Society of London, the author not being aware of the labours of Olbers in the same field. The methods of Olbers and Gauss supply all the requirements of the astronomer for computing the orbit of a comet, whether revolving in a parabola, an ellipse, or a hyperbola.

The present century has been distinguished by the apparition of several great comets, the movements of which have formed the groundwork of elaborate researches. Of these, by far the most celebrated is Halley's comet, the passage of which through the perihelion occurred on the 15th of November, 1835.

* *Problema longè difficillimum.*

Several years previous to this time the Academy of Turin proposed as the subject of a prize the computation of the disturbing influences exercised upon the comet by the various planets, with a view to ascertain the exact elements of the comet at the time of passage of the perihelion. The prize was won by Damoiseau, who fixed for the return to the perihelion the 16th of November, 1835.

Results closely agreeing with this were obtained by Pontecoulant, Rosenberger, and Lehmann. The comet was first seen at Rome on the 5th of August, and it became a conspicuous object to the naked eye during the first fortnight of the month of October. Weichmann, having subjected to a profound discussion the totality of the observed places of the comet, as determined by various astronomers during the period of its visibility, employed the results in computing the elements of the comet's orbit, and hence found that it actually passed the perihelion on the 15th of November.

The researches of Encke on the comet which bears his name are celebrated for the indications which they offered in favour of the existence of a resisting medium. This comet, I may state, is remarkable for the shortness of its time of revolution, which amounts to only little more than three years. Now, Encke found that each successive revolution is gradually become shorter, to the extent of about two hours. But the theory of gravitation gives no account of any such alteration in the time of revolution. The central action of the sun would of itself cause the comet to revolve in an ellipse, the elements of which would be invariable; and the disturbing action of the planets would have merely the effect of sometimes retarding and sometimes accelerating the passage of the perihelion. Now, Encke found that, when the disturbing action of the planets was strictly taken into account, there remained in every instance an outstanding acceleration; and he was led, in consequence, to conclude that there existed in the planetary regions a medium which offered a resistance to the comet in the course of its orbital motion. It is easy to perceive that the effect of such a resisting medium would be to shorten the time of each successive revolution of the comet; for, the tangential motion being thus diminished, the sun's force would become more influential, and the comet would thus be gradually drawn to the sun. Now, the nearer a body is to the sun, the shorter is its time of revolution. Möeller, a Swedish astronomer, has found in the movements of the comet of Faye indications of a similar resistance. It is clear that if such a medium does exist, its influence will be more perceptible on small masses of bodies, like comets, than on solid accumulations of matter,

like the planets. However, the theory of Encke cannot be accepted as an established fact, in the absence of more extensive indications of the existence of a resisting medium than have hitherto been discovered.

The comet of Encke is remarkable for enabling astronomers to determine the mass of the planet Mercury with greater precision than is attainable by any other method. Its orbit at one part approaches very closely to the orbit of the planet, and the result consequently is, that the two bodies occasionally approach very near to each other. The planet tends in this manner to disturb considerably the movement of the comet; for, although its mass is small, it acts with considerable force, in consequence of the short distance which separates it from the disturbed body.

The great excentricity which generally characterises the orbits of comets has the effect of making those bodies approach, in several other instances, very near to some of the planets. A memorable illustration of this is furnished by Lexell's comet, which had been invisible down to 1767; but, having approached very close to Jupiter in that year, was thrown into a new orbit, and rendered visible, the time of revolution being little more than five years. But, in 1779, the two bodies having again approached very near to each other, the powerful attraction of the planet threw the comet into a new orbit, and again rendered it permanently invisible.

I have referred to the approach of a comet to a planet, and the consequences resulting therefrom; it might similarly be expected, since comets traverse the planetary regions in all directions and at all inclinations, with respect to the plane of the ecliptic, that comets would approach very near to the earth. This contingency has occurred in several very notable instances. There is a comet known as Biela's comet, which revolves in an elliptic orbit, the time of revolution being somewhat more than six years. This comet is remarkable for approaching very close to the earth's orbit in the course of its revolution round the sun; and on the occasion of the next passage of the perihelion, which occurred towards the close of the year 1832, great fears were entertained of the two bodies coming into collision. It was found, however, that while the comet passed through the place of near approach to the terrestrial orbit on the 29th of October, the earth would not pass through the corresponding place of its orbit until the 30th of November. Thus the fear of a collision happening between the two bodies was ascertained to be groundless.

If the earth had been travelling in the place of close proximity

to the comet's orbit on the 29th of October, it would have been certainly involved in the nebulosity which generally accompanies the nucleus of a comet; for it was found by calculations based on the apparent diameter of the head of the comet, that the radius of the nebulosity exceeded the distance between the orbits of the two bodies. It is plain, therefore, that the earth would have been involved in the nebulosity of the comet; but, from the smallness of the mass of the comet, we have no reason to suppose that, if such a contingency had been actually realised, any serious consequences would have resulted.

It remains for me, while upon the subject of comets, to make one or two allusions to the great comets which have appeared in the present century. Those to which I would allude are the comets of 1811, 1843, 1858, and 1861.

The great comet of 1811 will be memorable in history for its great splendour, the long interval of time during which it was visible, and the deep impression which its apparition produced on the public mind. Circumstances were especially favourable to its visibility. It was a circumpolar object. In other words, while it wheeled round, in virtue of the apparent diurnal motion of the celestial sphere, like all the stars in the firmament, it resembled a portion of the stars only in this respect, that it never set beneath the horizon. In the autumn of 1811, about the time of the passage of the perihelion, it shone with extraordinary splendour, and was visible every clear night during all the hours intervening between the evening twilight and the morning dawn. The observed movement of this comet was subjected to an elaborate investigation by Argelander, a German astronomer, who found it to revolve in an elliptic orbit, the time of revolution being 2888 years. The tail of this remarkable comet attained its greatest length about the middle of October, when it was found to extend to a distance of one hundred millions of miles. The breadth was fifteen millions of miles. The physical phenomena exhibited by this comet were carefully observed by the elder Herschel. I shall have occasion hereafter to advert to this subject.

The great comet of 1843 is remarkable for its splendour, and for having approached nearer the sun than any other comet recorded in history. It was best seen in the southern hemisphere. In Australia and at the Cape of Good Hope it appeared with extraordinary splendour. The tail, when at its greatest length, measured one hundred and fifty millions of miles in length. It passed the perihelion on the 28th of October. At the time of its passage

it was seen with the naked eye close to the sun. It is one of the few comets which have been so seen in the day time. The number altogether does not exceed six or seven. It suddenly appeared to the inhabitants of the British Isles on the 17th of March. The head had already set, but the tail appeared in the form of a huge beam of light, stretching over the south-west sky from the horison towards the constellation Lepus, and thus embracing an arc of nearly 40°. No intimation of its apparition in southern climes had reached the country (how different would be the state of matters in the present day); and astronomers were at first perplexed what to make of it, as the head, being close to the sun, was invisible after sunset. The orbit of this comet has been computed; but it is subject to a great deal of uncertainty, as it was visible to European astronomers only during a comparatively short interval of time.

The great comet of 1858 will be fresh in the recollection of most of you present. It was first discovered on the 2nd of June 1858. It became generally visible to the naked eye in September, and formed a conspicuous object in the heavens till the middle of October. The tail, when most completely developed, was found to extend over an arc of 40°. The absolute length measured about forty millions of miles. The time of revolution of this comet has been calculated to be 2138 years. The major axis or longest diameter of the orbit measured thirty-one thousand one hundred and seventy miles.

The great comet of 1861 is, no doubt, also well remembered by all of you. It suddenly burst upon the people of Europe upon the 30th of June; but it was seen in Australia as early as the 13th of May. The tail, even when viewed in the strong twilight, measured 70° in length. The head was brighter than a star of the first magnitude—indeed, it almost rivalled Jupiter. It rapidly diminished in splendour, but it continued to be visible for some months in telescopes.

No trace of ellipticity has been discovered in the orbit of this comet, nor have the elements been found to be identical with those of any other comet recorded in history. It has been ascertained, in fact, to be sensibly parabolic—in other words, the orbit, instead of being a closed curve, like an ellipse, has two infinite branches; and consequently, when the comet has once passed the perihelion it will never again return to it.

The introduction of meteors within the boundaries of astronomy forms one of the most striking results of modern scientific re-

search. Meteors are of three kinds,—aerolites or falling stones, detonating meteors or fireballs, and shooting stars. For a long time men of science were incredulous with respect to the falling of stones from the upper regions of the atmosphere to the earth's surface. The fact, however, has been established in the present century beyond all doubt. The great collection of meteorites in the British Museum will be amply sufficient to satisfy any person who may entertain doubts upon the subject. Some of these are of great size, weighing several hundreds of pounds.

Besides those meteorites, the fall of which to the earth has been established by the testimony of trustworthy eye-witnesses, there have been found, in various parts of the world, large metallic masses, the composition of which is similar to that of meteoric bodies, but which have no relation whatever to the structure of the rocks in the vicinity of which they have been found. The materials of bodies of this class are found to consist mainly of iron—whence they have been called masses of meteoric iron.

Fireballs consist of luminous globes which are seen to traverse the upper regions of the atmosphere with great velocity, and finally burst into fragments, accompanied in many instances with a loud explosion.

Shooting stars are so familiar to all of you, that I need not give any definition of this class of bodies. The number of shooting stars visible on a clear night, during any assigned interval of time, has been found to vary throughout the night. The frequency diminishes from evening till morning, attaining its minimum at six in the morning at those seasons of the year when the light of the sun does not interfere with their visibility. It has also been found that the number of shooting stars visible in any given time varies from month to month throughout the whole year. The number visible in an hour about midnight, when the sky is clear and the moon absent, has been found to be 12·8 for the month of August, while, again, for April the corresponding number is only 4·6, and the numbers for the other months have been found to vary between those two extremes.

A great number of apparitions of aerolites and fireballs are found recorded in the annals of modern science. Halley, in a paper which he communicated to the Royal Society on the great fireball of 1718, expressed the opinion that such apparitions arise from the combustion of agglomerations of matter in the upper regions of the atmosphere; but such a theory is manifestly incapable of accounting for the fall of immense quantities of matter to the earth. According

to Chladni, meteors of all kinds are cosmical bodies travelling in space in obedience to the sun's attraction. Sometimes they approach so near the earth as to be drawn by its attraction actually within the limits of the terrestrial atmosphere. When this occurs, their rapid motion produces a compression of the air opposed to them, and a consequent evolution of heat, which ignites them, and thereby renders them visible. This theory is now generally accepted as the true explanation of the phenomena presented by luminous meteors.

The earliest attempt to determine the heights of shooting stars was made in 1798 by two German physicists, Brandes and Benzenburg. They found, for a number of shooting stars, heights varying from 40 to 140 miles above the earth's surface, and velocities varying from 20 to 30 miles in a second. It was clear from these results that shooting stars are cosmical bodies traversing space with a velocity exceeding considerably the orbital velocity of the earth.

The results of subsequent researches on this subject have been in accordance with those arrived at by the physicists whom I have just now mentioned. The average height of a meteor has been found to be 70 miles, and the average velocity 30 miles in a second. The orbital velocity of the earth amounts to only 19 miles in a second.

It is well known that at certain times of the year meteors are seen in greater abundance than at other times. Every year, between the 6th and 13th August, there may be seen on any night of the week included between those two days a considerable fall of meteors, the number generally attaining its maximum about the 10th of the month. When their apparent paths are traced back, they are all found to emanate from a common point in the heavens, situated in the constellation Perseus. The November shower of meteors has also led to results in the highest degree interesting. This shower was first brought under the notice of the scientific world by Humboldt, who witnessed the phenomenon in South America in the year 1799, travelling with his friend, Bonpland. He thus describes the appearance observed by them:—"From half-past two the most extraordinary meteors were seen towards the east; they filled a place in the sky extending from the true east 30° towards the north and south. Bonpland relates that from the beginning of the phenomenon there was not a space in the firmament equal to three diameters of the moon which was not filled at every instant with fireballs and shooting stars."

The next great apparition of this shower occurred in 1832. Dawes, a well-known English astronomer, thus refers to it:—

"Grand as was the display of 1866, there was not a single meteor that would bear comparison with several that I saw at Ormskirk, on the 12th of November, 1832, on which occasion, notwithstanding a pretty bright moon, three or four cast a very dark shadow of my observatory towards the side on which the moon was shining. A very thick fog suddenly arose at about thirteen hours and a-half; but several of the country folk, going early with agricultural produce to Liverpool, said that though there was a terribly thick fog, yet it lightened all the way."

A still more magnificent display of meteors was witnessed in America in 1833. It was remarked on this occasion that the meteors seemed all to come from a common point in the heavens situated in the constellation Leo, near the bright star γ Leonis.

The great apparition of November, 1866, which was so well seen in the British Isles, will be memorable for its splendour. In the following year the shower was also witnessed in America under very favourable circumstances. In 1868 the shower was again seen in Europe, Africa, and America; but the numbers visible have since been gradually diminishing in each successive year.

I shall now refer briefly to the striking results which have been obtained by researches on the phenomena presented by this meteoric group. In 1864, Professor Newton, of Yale College, U.S., was led to investigate the subject. By a careful examination of the recorded appearances of meteors contained in the annals of the Chinese, the Arabs, and the writers of the Middle Ages, he came to the conclusion that the shower recurs at intervals of $33\frac{1}{4}$ years. He found, however, that it occurred later and later at the close of each successive cycle. He ascertained in this manner that the intersection of the stream of meteors with the plane of the earth's orbit is not fixed, but advances yearly upon the ecliptic at the rate of fifty-two seconds relatively to the stars. He further remarked that the phenomena of the shower might be satisfied by assuming that the meteors constituted a stream of considerable length revolving round the sun in an elliptic orbit. The time of revolution of the orbit admitted of five different values. In one year the meteoric stream might describe either two whole revolutions plus $\frac{1}{33}$, or two whole revolutions less $\frac{1}{33}$, or it may describe one whole revolution plus $\frac{1}{33}$, or one whole revolution less $\frac{1}{33}$; or, finally, it may describe neither more nor less than $\frac{1}{33}$ of a revolution. In other words, the time of revolution of the meteoric stream would

be 180 days, or 185·4 days, or 354·6 days, or 376·6 days, or, finally, it might be 33¼ years. On any of these suppositions the phenomenon would be characterised by a recurring cycle of 33¼ years. It was clear, also, that the stream was of considerable length, since it appeared from the recorded displays that the phenomenon might be visible in two consecutive years.

We all remember how brilliantly this theory was verified in the case of the great November shower of 1866. A similar verification was obtained in the following year in America, where the shower appeared with extraordinary splendour. Professor Newton had remarked very sagaciously, that although the appearances of the shower would be satisfied by any one of the five suppositions he advanced, still the annual motion of the node, which he had found to amount to 52 seconds of arc would be different for each supposition, and he remarked that this would furnish a sure criterion for deciding what was the true form of the orbit in which the meteors revolve. Professor Adams, taking up this view of the subject, has computed the motion of the node of the meteors, and he has found that while in the case of the first four suppositions the computed motion of the node was in each case largely different from the observed motion, on the other hand the motion of the node computed on the supposition of a period 33¼ years agreed exactly with the value which had been deduced from the recorded apparitions. It was clear then that the period 33¼ years must be regarded as the true period.

It has been surmised that this great group of meteors originally formed an agglomerated mass, which has been drawn out into the form of a long stream by the sun's attraction, and that this process of dislocation will continue to go on until the matter of the stream shall be uniformly distributed along the entire contour of the orbit. When that occurs, the cycle will disappear, and there will be a uniform display of the meteors in each successive year. This contingency appears to have already occurred in the case of the August meteors, the apparitions of which, one year compared with another, are found to be generally characterised by the same features as regards richness of display.

Schiaparelli first drew attention to the remarkable fact that the orbit of the August meteors coincides almost exactly with the orbit of the third comet of 1862; and shortly afterwards Peters pointed out the existence of a similar identity between the orbit of the November meteors and the orbit of the first comet of 1866.

We have here a wide field of research opened up, which is *destined* doubtless to lead one day to important results.

I have thus far attempted to give some account of the researches of astronomers in connexion with the principles which govern the movements of the celestial bodies. I now proceed to consider the question of their absolute distances from the earth. The Greek astronomers, who made considerable advances in the theory of the movements of the heavenly bodies, were unable to solve, even approximately, this most interesting question, if we except the single case of the moon's distance. This arose from the imperfect state of their observations, which were altogether inadequate to form the groundwork of so delicate an inquiry. Even down to a comparatively recent period, astronomers could merely assign an inferior limit of distance within which the celestial bodies were certainly not situated. How far the distance of the nearest of them exceeded this limit they were absolutely unable to assert. It was only after the invention of telescopic sights, and the application of the micrometer to divided instruments, that the observations of the heavenly bodies acquired such a degree of refinement as to enable astronomers to derive from them positive results with respect to the absolute dimensions of the planetary system. The distance of the sun from the earth is the fundamental element to be considered in this inquiry. A knowledge of the distance of the moon from the earth leads to no ulterior results. The distance of the sun, on the other hand, puts us at once in possession of the distances of all the planets, satellites, and comets, from the sun, as well as the distances of the various satellites from their respective primaries. Furthermore, the diameter of the earth's orbit, being thus known, serves as a groundwork for investigating the grand problem of the distances of the fixed stars. Now, the question of the sun's distance from the earth depends upon a knowledge of two facts—the length of the earth's radius, and the angle included by two imaginary lines connecting its extremities with the centre of the sun one of these lines being perpendicular to the terrestrial radius, or in other words, the solar parallax. The earth's radius was determined in the seventeenth century with sufficient precision for the solution of this problem. The determination of the solar parallax is, however, attended with immense difficulty. This arises from the smallness of the angle to be determined, and the peculiar difficulties which affect observations of the sun's place. Accordingly, astronomers have attacked the problem indirectly. The method consists in observing such of the planets as approach nearest the earth. Now, there are two bodies which have been found suitable for this purpose, the planets Mars and Venus, the former revolving immediately beyond, the latter

immediately within the earth's orbit. When Mars is in opposition, he occasionally approaches very near to the earth. The distance between the two bodies on some of such occasions does not, in fact, exceed thirty millions of miles. This arises from a curious relation existing between the orbits of the two planets, in so far as their aphelia or perihelia are concerned. The aphelion of the earth is turned nearly towards the perihelion of the planet, and the result consequently is, that every fifteen or seventeen years the two bodies approach very closely to each other. Now, the nearer a planet is to the earth, the more considerable is its parallax, and consequently the more favourable is the occasion for ascertaining the value of that element. But when the parallax of the planet has been thus determined, the relative distances of the planets from the sun being already known, we readily arrive at a knowledge of the solar parallax. The earliest determination of the value of the solar parallax was based upon observations of Mars; but the method soon fell into disuse, in consequence of the greater confidence placed by astronomers in another method, to which I shall now briefly refer. This is the celebrated method founded upon the transit of Venus across the sun's disc. On certain rare occasions, the planet may be seen as a small dark round body traversing the sun's disc. These transits usually occur in pairs. The first two which attracted the attention of astronomers occurred in 1631 and 1639. The next two transits occurred in 1761 and 1769. Halley, the renowned English astronomer, had pointed out the advantages which the observations of the transits would afford for obtaining an accurate value of the solar parallax; and he strongly urged the next generation of astronomers not to allow so precious a phenomenon to pass unobserved. It is well known that the principal nations of Europe fitted out expeditions to various parts of the world for the purpose of observing those transits. The observations were actually made, and various values of the solar parallax, differing more or less from each other, were derived from them; but the value which ultimately superseded all the others was assigned by Encke as the result of careful discussion of the totality of the observations of both transits. The value which the German astronomer obtained in this instance for the solar parallax amounted to 8·″57, indicating that the distance of the sun from the earth amounts to 95 millions of miles. The recent researches of astronomers have led to the discovery of the important fact, that the value of the solar parallax as assigned by Encke was sensibly too small, or, in other words, that the distance of the sun from the earth was too great.

The earliest indication of the necessity for such an alteration in the value of the solar parallax is contained in a letter from Hansen to the Astronomer Royal, published in the Monthly Notices of the Royal Astronomical Society for November, 1854. Hansen was guided in this matter by his researches in the lunar theory. He subsequently assigned 8·″95 as the value derivable from his researches. This reduces the sun's distance from the earth about a $\frac{1}{30}$th part, making the real distance to be only 91½ millions of miles. A few years afterwards, in 1858, Le Verrier was led to assign 8·″916 as the value of the solar parallax, derivable from his planetary researches. In 1863, Stone obtained 8·″932, and Winnecke obtained 8·″964 from their respective discussions of the observations of Mars in 1862. Still more recently, Stone, by a discussion of 2075 Greenwich observations of the moon, executed between the years 1848 and 1866, obtained 8·″850. These different results all presented a remarkable accordance with each other, and they received a most striking confirmation from a totally different quarter. It had been hitherto assumed that light travels in space with a velocity amounting to 192,000 miles in a second. This result was obtained by an astronomical measurement of the time which light occupies in traversing a diameter of the earth's orbit. The time being given, and the radius of the earth's orbit being given, the velocity of light is hence obviously deducible. It is clear, then, that if we assume the earth's radius to be too great, the resulting value of the velocity will be correspondingly too great. Now, Foucault, a French physicist, found, by a most ingenious laboratory experiment, that the assumed velocity of light was too great. He found, in fact, that the velocity amounts to only 298,000 kilomètres in a second. This would indicate a value of the sun's parallax amounting to 8·″86, which agrees very nearly with the results derived from astronomical sources. Finally, Stone, having subjected to a re-discussion the original observations of the transits of Venus, obtained 8·″91 as the value deducible from them. Thus a complete accord everywhere prevails, and no longer any doubt can remain, that the solar parallax is about $\frac{1}{30}$th greater than the value which Encke had assigned to it.

The approaching transits of Venus in 1874 and 1882 are looked forward to by astronomers with intense interest. Steps have already been taken by the principal nations of Europe to observe the transit of 1874 with the aid of all the appliances of modern science. The transit of 1882 will be eminently favourable for observation in America. Between those two important phenomena there will

occur one of those oppositions of the planet Mars, which, as I have already stated, are peculiarly favourable for the same purpose. We have thus three phenomena—the transit of Venus in 1874, the opposition of Mars, 1877, and the transit of Venus in 1882—all favourable for obtaining a fresh determination of the value of the solar parallax; and it cannot be doubted that the various efforts which they will call forth on the part of astronomers will lead to a value of that important element still more trustworthy than any yet arrived at.

The Astronomer-Royal has made all the necessary arrangements for observing the transit of 1874, in so far as this country is concerned. The contemplated observing stations are Kerguelen's Island, Woahoo, Auckland in New Zealand, Alexandria, and Rodriguez.*

I proceed to make a few remarks on the progress of observational astronomy in the present century. The observations of the stars have in every age constituted the true groundwork upon which the vast superstructure of astronomy has been reared. Now, we may view the observations of the stars in two very distinct lights. We may consider their places in the celestial sphere as so many reference points for determining many of the fundamental facts of astronomy,—facts which would acquire for the stars a paramount importance even although they possessed no claim upon other grounds to our attention. But independently of such considerations, the stars on their own account offer a high degree of interest. Viewed in this light, the researches in connexion with the stars constitute one of the most fascinating branches of astronomical science. I shall presently have occasion to refer to the progress of stellar astronomy. In the meantime I would allude briefly to the observations of star places executed in the present century, viewing them as reference points in the heavens, and independently of any intrinsic considerations as regards the stars themselves.

It will not be difficult to appreciate the vast importance of a collection of well determined star places. It is by their aid that the astronomer is enabled to ascertain, with all the precision which the existing state of science requires, the positions of the various bodies of the planetary system, and in this manner to arrive at a knowledge of the laws of their movements. In connexion with such observations, the refinements of modern astronomy have forced upon

* It is intended to employ photography in the observation of the approaching transit of 1874; Mr. De La Rue has undertaken to superintend the arrangements. To Proctor is due a very able investigation of the various circumstances relating to the transits of 1874 and 1882.

the consideration of the astronomer a class of subjects, each of which, from its vast importance, may be regarded as a distinct branch of science.

You are all aware that a ray of light coming from a celestial body is deflected from its original course in consequence of the refraction which it undergoes in passing through the earth's atmosphere. Hence arises a displacement of the apparent position of the object. Now, the discovery of the magnitude of this displacement, and of the laws by which it is regulated, is founded upon the discussion of a vast number of star places observed in every part of the heavens, and under every condition as regards the density, temperature, and meteorological conditions of the atmosphere.

So, again, with regard to the grand phenomenon of the precession of the equinoxes. The place of a star in the celestial sphere may be, and usually is, determined by measuring its distance from two great circles of the celestial sphere, one of which is represented by the equator, and the other by a circle perpendicular to it, and passing through the points of intersection of the equator and the ecliptic. Now, if we compare a celestial globe made in the beginning of this century with a globe made in the present day, we shall find that the intersecting points of the equator and the ecliptic are not coincident in both cases. It will be ascertained, for instance, that the vernal equinox, which is represented by one of the points of intersection of the two circles, when viewed upon the globe of our own day, has retreated about 1° upon the ecliptic in a retrograde direction, or contrary to the order of the signs, as compared with the position laid down upon the globe constructed in the beginning of this century. Clearly, then, the points of intersection of the equator and the ecliptic have shifted their positions during the intermediate period. Now, it has been found that this displacement of the equinoxes is continuous—in other words, it goes on from age to age, until, finally, after the lapse of about 26,000 years, it makes the complete circuit of the ecliptic; and since the circles of reference to which I have alluded undergo a corresponding change, the place of a star, if determined at the beginning of this century by measuring its distance from those circles, will not be comparable with the place of the same star similarly determined in our own day, unless we take into account this continual shifting of the circles of reference. But the magnitude and laws of this phenomenon can only be discovered by the discussion of a vast number of star places, as affected by it from age to age, and under every variety of circumstances. To give a third illustration. The light from a celestial

body is not transmitted instantaneously. It traverses space with a velocity of 186,000 miles in a second. This fact, combined with another fact, the annual motion of the earth around the sun, gives rise to a displacement of the apparent positions of the stars, in virtue of which they all seem to describe a small ellipse in the heavens. Now it is an object of the utmost importance that the elements of this ellipse should be ascertained with the most rigorous precision. But here, again, an extensive collection of well determined star places constitutes the indispensable basis of inquiry.

I have thus endeavoured to point out the vast importance attached by astronomers to the determination of the places of the stars, irrespective of the claims which the stars themselves may urge upon their attention. If anything were wanting to impress this circumstance more deeply on your minds, I would quote the words of the illustrious astronomer whose recent decease has created so great a blank in the scientific world.

"The stars," says Sir John Herschel, "are the landmarks of the universe, and, amidst the endless and complicated fluctuations of our system, seem placed by their Creator as guides and records, not merely to elevate our minds by the contemplation of what is vast, but to teach us to direct our actions by reference to what is immutable in His works. It is, indeed, hardly possible to over appreciate their value in this point of view. Every well determined star, from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor, a point of departure which can never deceive or fail him,—the same for ever and in all places, of a delicacy so extreme as to be a test for every instrument yet invented by man, yet equally adapted for the most ordinary purposes; as available for regulating a town clock as for conducting a navy to the Indies; as effective for mapping down the intricacies of a petty barony, as for adjusting the boundaries of transatlantic empires. When once its place has been thoroughly ascertained and carefully recorded, the brazen circle with which that useful work was done may moulder, the marble pillar totter on its base, and the astronomer himself survive only in the gratitude of posterity; but the record remains, and transfuses all its own exactness into every determination which takes it for a groundwork, giving to inferior instruments, nay, even to temporary contrivances, and to the observations of a few weeks or days, all the precision attained originally at the cost of so much time, labour, and expense."

It is obvious from these remarks that the construction of a catalogue of stars is an object of fundamental importance in astronomy.

The labours of this kind executed in the present century far transcend any previous efforts in the same field. Our great national institution at Greenwich maintains its pre-eminence in this respect. Star catalogues of the highest importance, however, have emanated from other sources. I may mention in connexion with such labours the names of Piazzi, Bessel, Argelander, Struve, and Rümker on the Continent, and those of Groombridge, Johnson, Robinson, and Carrington in our own country.*

The instrumental resources available to the astronomer have received a marvellous extension since the beginning of the nineteenth century. The mural quadrant which had been so long used for meridional observations had to give way to the mural circle, which in its turn is now being rapidly supplanted by the transit circle. The only achromatic refracting telescopes of the last century were those made by Dollond, which, although of exquisite quality, did not in general exceed five or six inches in aperture. There are now in use, for astronomical purposes, refracting telescopes of apertures varying from six to eighteen and twenty-five inches. The reflecting telescopes constructed by the elder Herschel were undoubtedly marvels of ingenuity and practical skill, but they have been surpassed in our own day by the magnificent instruments of Lord Rosse and Mr. Lassell. Furthermore, by the inventions of photography and spectrum analysis, a veritable revolution has been effected in the instrumental means applicable to the study of astronomical physics. Nor should I omit to mention the aid which the astronomer has derived in recent years from the subtle agency of electricity. The transit of a star over the meridian is now recorded electrically with a precision which surpasses the utmost skill attainable by the eye and ear method heretofore used. By electricity also the difference of longitude of places on the earth's surface is now determined with a trustworthiness and an accuracy which was not attainable by any previous method. The origination of these remarkable applications of electricity is due to the American astronomers. One of the earliest results of the laying down of the Atlantic Cable was an electric determination of the difference of longitude of the Observatories of Greenwich and Cambridge, U.S. Still more recently the same method has been employed for determining the difference of longitude between the American Observatory just

* Neither Groombridge nor Johnson lived to witness the publication of their respective Catalogues. Groombridge's Catalogue was edited by Airy. The Catalogue of Johnson was edited by his successor, Main, the present Director of the Radcliffe Observatory.

mentioned and the city of San Francisco on the Pacific coast. In this case the route traversed by the electric current extended to a distance of 3550 miles; and if we combine the result with the difference of longitude between Greenwich and Cambridge, we obtain the electric determination of an arc of parallel extending over more than eight hours of longitude.

One of the most prominent characteristics of the astronomy of the nineteenth century consists in the attention devoted to the reduction of observations. The era of astronomical observations comparable in point of precision with those of the present day, and therefore suitable as a groundwork of modern research, dates from the middle of the last century, when Bradley commenced his great career at Greenwich. But although valuable observations of the sun, moon, planets, and stars were made by Bradley and his successors, they were given to the world in an unreduced form, and were therefore not immediately available as materials of research. To Bessel is due the complete reduction of Bradley's star observations, and the construction from them of a catalogue of 3112 stars, which was published in 1818. Still later the reduction has been effected of the vast mass of observations of the moon and planets which had been made at Greenwich during the period included between 1750 and 1830. This stupendous two-fold task was executed at the public expense, upon the recommendation and under the superintendence of the present Astronomer-Royal, who, I ought to remark, was the first British astronomer who set the example of publishing observations in a completely reduced form.

I have already referred to the brilliant results which have been achieved in meteoric astronomy. We must not omit to acknowledge how much of this is due to the persevering and skilful observation of meteoric phenomena by Heis, Alexander Herschel, Greg, and others, who have devoted especial attention to this interesting subject.

While the facts which form the basis of astronomical research have received a scrutiny more searching and profound than had been heretofore applied to them, the methods of investigation have been characterised by a corresponding increase of power, comprehensiveness and refinement. In this vast field of astronomical science, the illustrious Bessel stands without a rival. How justly has been applied to him the language suggested by the munificence of the Roman Emperor—

"Lateritiam accepit, marmoream reliquit."

The works of this great investigator, while they constitute at once *the type and the glory* of German modern astronomy, will go

down to future generations to be studied as classical models of research by the astronomers of all countries.

I have referred to the inestimable value of the telescope in enabling the astronomer to determine the apparent places of the heavenly bodies with a degree of precision far surpassing what had previously been arrived at. Without the aid of this powerful appliance the results relative to the movements of the celestial bodies would have been almost a complete blank, and Newton would have failed to achieve his grand discovery of the law of gravitation. I have now to allude to the use of this and other more recent inventions in leading us to a knowledge of the physical constitution of the celestial bodies. It is impossible in the present day to form any adequate conception of the profound impression produced throughout the whole scientific world by the announcement of the discoveries made by Galileo when he turned his tiny instrument, the fruit of his own invention, to the heavens. The moon's surface appeared everywhere diversified with hills and valleys, like the earth. The dark shadows of the mountains were seen gradually increasing in length as the sun continued to descend towards the moon's horizon; while, again, beyond the boundary of light and darkness, there were seen bright specks of light, indicative of mountain peaks, which caught the first gleam of the rising sun, or the last of the setting sun, while the plains below were buried in darkness. The planet Venus exhibited phases like the moon, shewing clearly thereby that she was an opaque body like the earth. The planet Jupiter presented an aspect still more striking, exhibiting four satellites revolving round him in the same way as the moon revolves round the earth. All these facts furnished triumphant evidence in favour of the Copernican system of the universe, the question with respect to the truth or falsehood of which was agitating the minds of scientific men. The sun, when viewed with the telescope, presented an unexpected aspect. It was an idea of the ancient philosophers that the sun was totally different in its nature from terrestrial bodies—that it had a uniformly bright surface—while terrestrial bodies presented an opaque surface, diversified in various ways. We may judge, therefore, the consternation with which they received the announcement that the sun's surface was diversified with dark spots of irregular form and magnitude, which appeared to be subject to incessant fluctuations when viewed from day to day.

To Alexander Wilson, the original occupant of the Chair of Astronomy in the University of Glasgow, we owe the discovery of the important fact that the spots are cavities in the sun's surface.

The elder Herschel adopted the views of Wilson in this respect, and explained the phenomena of the spots by means of two concentric envelopes encompassing the dark body of the sun.

The study of the sun's surface has continued to occupy the attention of astronomers since the epoch of Galileo's discoveries; but it is chiefly in the present century that the greatest advance has been made in this interesting branch of celestial physics. To bring under the dominion of law the apparitions of the solar spots was long considered a hopeless task, so irregular and capricious did they seem. To Schwabe we owe the discovery of a law regulating the frequency of the spots on the solar disk, the result of a most persevering scrutiny of the sun's surface, extending over a period of thirty years. He found that the interval included between two maxima or minima of groups of spots is about ten years. Wolff, by a more extensive comparison of recorded observations of the spots, determined the period to be 11·1 years. Sabine remarked that the periods found by Schwabe agree very nearly with the period of eleven years which Lamont had found for the diurnal variation of the magnetic needle. Taking into account the more correct value obtained by Wolff, the agreement may be said to be identical. I ought to state that Wolff and Gautier had, independently of Sabine, remarked the coincidence of the solar spot and magnetic periods. Observations derived from other sources seem to indicate some close physical connexion existing between solar phenomena, the phenomena of terrestrial magnetism, and the phenomena of auroras. A most valuable series of observations of the solar spots, extending over a period of seven years, has been executed by Carrington. Finally, during several years past, a photo-heliograph has been established at Kew, under the superintendence of De la Rue, with which sun pictures have been taken on every available day. The results of those observations have been published in the *Transactions* of the Royal Society of London.

By a discussion of his observations, Carrington has discovered the important fact that the velocity of the spots on the solar surface is variable, being greatest at the equator, and diminishing thence towards the poles. He explains in this manner the very discordant values obtained by different astronomers for the time of rotation of the sun upon its axis.

I shall now advert briefly to the remarkable phenomena observed during eclipses of the sun, and the conclusions therefrom deduced. In 1842 there occurred a total eclipse of the sun, which was visible in the southern parts of Europe, and was carefully observed by *some of the most* celebrated astronomers then living. During the

totality, a phenomenon was visible which excited much interest. Several reddish coloured prominences were seen apparently attached to the moon's limb. But a subsequent reference to earlier eclipses shewed that a similar phenomenon had been observed on more than one previous occasion. The question which first offered itself in connexion with these phenomena was this: Are they solar or lunar appendages? A closer investigation of the question shewed incontestably that they belong to the sun.

Besides the prominences, there were seen by several of the observers of the solar eclipse of 1842 a band of red light, having a serrated outline, and apparently attached to the moon's limb. In this case, again, it was found that a similar phenomenon had been seen during many previous eclipses. In the winter of 1850–51, having had occasion to examine the various phenomena seen during solar eclipses, as recorded in the annals of astronomical observation since the beginning of the seventeenth century, I found that, whether in the case of total, or annular, or partial eclipses, there existed abundant evidence of this arc of red light being visible, *and that, too, invariably at the part where the moon just covered or slightly overlapped the sun's disc.* Furthermore, the arc was visible at the upper or lower limb, the eastern or western limb, or any intermediate part, as the case might be, the question of its whereabouts depending on the position of the observer with respect to the defining lines of the moon's shadow or penumbra. I accordingly announced that, in addition to the two envelopes which Herschel assigned to the sun, there existed *a third envelope*, which was superposed upon the photosphere.* Shortly afterwards a similar conclusion was arrived at, quite independently of me, and also of each other, by Swan and Littrow, who were both observers of the total eclipse of 1851. The spectroscopic observations of the sun have also furnished evidence of the existence of such an envelope. As soon as Lockyer succeeded in observing the prominences in full sunlight, he discovered traces of this envelope all round the sun's disk.† Secchi and Jannsen also readily obtained indications of the same phenomenon.

The total eclipse of 1860 is memorable for the application of photography to the delineation of the phenomena visible during solar eclipses. On that occasion photographs, exhibiting the phenomena visible during the totality, were obtained by De la Rue and Secchi,

* *History of Physical Astronomy*, p. 400; *Monthly Notices of the Royal Astronomical Society*, vol. xxxii. p. 33.

† Lockyer at this time had not been aware of any previous researches on the subject.

which, independently of other considerations, furnished a most satisfactory proof of the prominences being solar and not lunar appendages.

The grand discovery of Kirchhoff respecting the reversability of the spectra of incandescent bodies, constitutes a new epoch in the history of astronomical physics. We now learn, for the first time, the materials of which the celestial bodies are composed. We find that in the atmosphere of the sun there are indications of hydrogen, sodium, magnesium, iron, calcium, nickel, chromium, copper, zinc, barium, and probably strontium, cobalt, and cadmium.

The solar eclipse of 1868, which was visible in India, is memorable for being the first to which the spectroscope was applied, and for leading to the observation of the spectra of the prominences in daylight—a step, it is well-known, due independently to Jannsen and Lockyer. Shortly afterwards Huggins was enabled to advance a step further. Hitherto it was possible merely to feel the prominences by means of their spectra. Huggins succeeded in seeing the actual form of a prominence. His *modus operandi* has been since improved upon; and it is now possible to observe the forms of the prominences daily, and to mark their positions on the circumference of the sun's disk. Lockyer, Secchi, Respighi, Zöllner, and Young have been especially distinguished by their researches in this department of spectroscopic observation. Professor Young has succeeded in taking a photograph of a prominence in full sunlight. The chromosphere, as the envelope superposed upon the photosphere has been termed, and the prominences which emanate from it, have been found to consist mainly of hydrogen.

Beyond the chromosphere there appears around the sun, during a total eclipse, a luminous circle, which has given rise to much discussion in modern times. This phenomenon has been termed the *corona*. During the eclipse of 1868, an attempt was made to obtain the spectrum exhibited by it, but only with partial success. It would appear, however, from spectroscopic observations of the corona, made during the eclipse of 1870, that the spectrum consists merely of a bright line, indicating an agreement with the spectrum of auroral displays.

It is to be hoped that the eclipse of this year will settle some of the more interesting points connected with this mysterious phenomenon.*

* This eclipse (December 12, 1871) has been well observed in India by Tennant, Pogson, Lockyer, Jannsen, Respighi, and others. The existence of the green line in the spectrum of the corona has been confirmed, also certain fainter bright lines indicative of hydrogen, and furthermore, certain of the dark lines of the *ordinary solar spectrum*, especially that of sodium (D). Jannsen, whom I here

On the subject of the planets I must confine myself to one or two remarks:—During the oppositions of 1860, 1862, and 1864 admirable delineations of the physical aspect of the planet Mars were executed by several observers. By a comparison of some of these with a drawing of the planet, executed by Hooke in the year 1666, Proctor was enabled to determine the time of rotation of the planet with a marvellous degree of precision. The Saturnian system has been enriched by the discovery of a third ring of a dusky hue, encompassing the planet within the two bright rings, and of an eighth satellite revolving beyond the seven previously known to exist. The former of these discoveries is due to the independent observations of Bond and Dawes, the latter to the independent observations of Bond, Lassell, and Dawes. The body of the planet has been seen through the substance of the dark ring. This has been satisfactorily accounted for by the result of a mathematical investigation of the dynamical condition of the rings due to Clerk Maxwell who has established beyond doubt the important fact that the ring is not a continuous substance, but consists of a congeries of minute bodies or meteorites revolving round the planet. The elder Herschel was led to conclude from his observations that the figure of the planet is not strictly elliptical. This notion, however, has been proved independently by Bessel and Main to be wholly unfounded. With respect to the planet Uranus, its discoverer established the existence of two satellites (Oberon and Titania), and suspected four others which have not been seen by subsequent observers. Two other satellites, however, have been discovered, one (Umbriel) by Otto Struve and Lassell—the other (Ariel) by Lassell, to whom, also, we owe the discovery of a satellite revolving round Neptune.

With respect to the physical constitution of comets, facts have

quote, thus sums up the result of his observations :—"Beyond the cosmical matter, independent of the sun, which must exist in the vicinity of that body, the observations demonstrate the existence of an extensive but excessively rare atmosphere, having hydrogen for its base, extending far beyond the chromosphere and the prominences, and fed by the matter of which the latter are composed—matter ejected with immense force through the photosphere, as is proved by every-day observations. The rarity of this atmosphere at a certain distance from the sun must be excessive. Its existence, therefore, is not inconsistent with the observations of certain comets which, in the course of their respective revolutions, approach very close to the sun." I may state that the idea of the corona being fed by matter ejected from the sun had been already put forth, and ably discussed by Proctor in several papers, which have been published in the Monthly Notices of the Royal Astronomical Society.—(*Appended Note.*)

been observed in recent years which have given rise to much discussion. On the occasion of the return of Halley's comet to the perihelion, a careful examination of the head shewed it to be violently agitated as the comet approached the sun. A stream of light was seen to issue from the nucleus, and then to curve back, as if impelled by some intense force, in the opposite direction. A somewhat similar phenomenon was found to have presented itself during the visibility of the comets of 1680 and 1682. But it was best seen in the case of the great comets of 1858 and 1861. The comet of 1858 exhibited eight envelopes, which arose in succession from the head, and gradually melted away into the tail. In the case of the comet of 1861, there were observed as many as eleven envelopes, a fresh envelope ascending from the head every second day. No satisfactory account has been given of the origin of these envelopes, nor of the other phenomena which comets exhibit when they approach the sun. Thus far, however, an advantage has been gained. The phenomena exhibited by great comets, on their approach to the sun, have been observed with a fulness and precision previously unknown; and it is to be hoped that they will ultimately form the groundwork of reliable conclusions relative to the physical constitution of this mysterious class of bodies.

The spectroscopic observations of comets by Huggins would seem to indicate that they are mainly composed of carbon.

I have already referred to the singular analogy which has been established between comets and meteors. The first comet of 1866 must be held to be the advanced guard of the November swarm of meteors. That the head of a comet consists of a group of discrete bodies, is indicated by various passages in the records of cometary observations. We may therefore regard it as highly probable that the heads of comets consist generally of mere aggregations of meteorites. In conformity with this view, Tait has suggested that the variations of light observed in the head of the comet may be the result of collisions occuring among the constituent meteoric bodies.

If from the planetary system we extend our survey to the regions of the stars, we encounter everywhere phenomena of the highest interest and significance. I regret that, in consequence of the length to which this address has extended, I am unable to do little more than merely allude to this branch of the science. A striking characteristic of the planets consists in the circumstance that the variations of lustre which form so conspicuous a feature of their physical aspect

depend on their positions with respect to the sun. The stars, however, offer no such homage to the great luminary of the planetary system. It was clear, then, from this circumstance, combined with the fact of their always maintaining the same position relatively to each other, that the stars were to be considered as a class of bodies apart, having no immediate connexion with the planetary system, and situated in all probability far beyond its limits.

In every age there have appeared men of bold minds who were capable of rising to the idea of the stars being so many suns, each of them, as in the case of our own sun, forming the centre of a retinue of revolving worlds. One of the most remarkable of these was Giordano Bruno, an Italian philosopher of the sixteenth century. He propounded the doctrine that all the stars are equal in magnitude and splendour to the sun, and that an innumerable multitude of such bodies was distributed throughout the boundless regions of space. He maintained that we are not warranted in supposing them to be fixed with respect to each other, since their distance from the earth is so immense that it would be impossible to detect their motions, and he added that it could only be decided after a long course of observation whether the stars revolved round each other or what other motions they might have. It is the triumph of modern astronomy to have assigned a satisfactory solution of these grand questions.

One of the first fruits of the application of the telescope to the observation of the heavenly bodies consisted in the discovery of the fact that the celestial vault is everywhere strewn with an innumerable multitude of luminaries, the light of which is too faint to render them visible to the naked eye. The elder Herschel pursued this field of inquiry with the ability and perseverance which characterized all his labours, and arrived at results of a nature the most astounding. On one occasion he found that in the short interval of one quarter of an hour as many as 116,000 stars passed through the field of view of his telescope!

The determination of the quantity of light emitted by the fixed stars relatively to each other and to the sun, is a branch of inquiry which has occupied considerable attention on the part of the astronomers of the present century. The late Sir John Herschel, while residing at the Cape of Good Hope, executed a series of careful observations with the view of elucidating this important point, and arrived at results which, viewed in connexion with the element of stellar parallax, has led to some interesting conclusions as regards the absolute magnitude of the sun and the stars.

To ordinary observers of the heavens, the stars appear to stand out in marked contrast to the planets, by always exhibiting the same degree of brightness. This, however, turns out to be a hasty impression which is soon dispelled in many instances when the brightness of a star becomes a subject of careful scientific observation. A great number of stars have been discovered to be constantly varying in brightness, passing through a cycle of changes in periods of time which have been determined.

The vexed question of the parallax of the fixed stars has finally, in the nineteenth century, received a definitive solution. The star which hitherto has been found to have the largest parallax, is α Centauri, one of the brightest stars in the southern hemisphere, but even in this instance the parallax barely amounts to a second of space. In other words the nearest of the fixed stars is distant from the earth at least twenty billions of miles!

Many stars when viewed through the telescope are found to consist in reality of *pairs* of stars, the individual members of which are so close to each other that to the naked eye they appear as so many single stars. The discovery of the physical connexion of double stars constitutes one of the greatest triumphs of the genius of the elder Herschel. By a comparison of careful measures of the relative positions of the two stars, executed at different epochs of time, he established the important fact that in many instances the one star is gradually revolving round the other, and he even roughly assigned the periods of revolution of several of such stars. In the present century the discovery of new double stars, and the micrometric measurement of the relative positions of the constituent bodies have been prosecuted with great success by William Struve, the younger Herschel, and others. Furthermore, researches on their movements have resulted in the discovery of the important fact that the law of gravitation, as announced by Newton, rigorously holds good in the remote regions of the sidereal universe.

The stars, unlike the planets, appear to maintain constantly the same relative position. Hence the appellation of *fixed* stars which has been applied to them. The refined observations of modern astronomers have, however, disclosed the fallacy of this view. In a great number of recognised instances, and probably in the case of all of them, the stars have been discovered to be affected by a very minute motion, in virtue of which are they gradually but slowly shifting their apparent places in the celestial sphere. To Main we owe an elaborate investigation of the proper motions of 1167 stars, derived from a comparison of *Bradley's* observations with those of Airy's twelve-year catalogue.

These fundamental results have subsequently received an important accession from the labours of Stone. The question arises, Are those apparent motions the result of real motions affecting the places of the stars, or are they merely an optical effect produced by a motion of translation of the solar system in space? The researches of astronomers have led to the conclusion that the phenomena are attributable to a combination of both those causes, and furthermore have assigned the region of the celestial sphere towards which the sun, with his attendant retinue of planets, satellites, comets, and meteors is travelling. The most recent investigation of this great problem is due to Airy, whose researches were based upon Main's proper motions, and were conducted in accordance with a method of solution altogether different in principle from that employed by any of his predecessors. Curious enough, however, the final result agreed very closely with that derived from previous researches, although it must be acknowledged that to the astronomer this interesting problem still appears to be surrounded with difficulties. The researches of the elder Herschel on the distribution of the stars in space have been succeeded in the present century by those of William Struve, who arrived at the conclusion that there is a gradual condensation of stars towards the plane of the Galactic Circle. The more general case of the distribution of stars and nebulæ has been recently taken up by Proctor, who sets out upon principles different from those employed by Herschel, but which, when fully worked out, promise to lead to interesting and important conclusions. The spectroscope has been applied to observations of the stars with signal success. Huggins and Miller led the way in these researches. From them we learn that the spectra of the stars in general resemble the spectrum of the sun; that in Sirius we find indications of the existence of sodium, magnesium, iron, and hydrogen; that the spectrum of α Lyræ reveals the existence of sodium, magnesium, and iron, and so with a number of other stars. Secchi has prosecuted this branch of spectroscopic astronomy with distinguished success. He has classified the stars into four perfectly distinct types, depending upon the spectra which they exhibit. The stars of the first type are Sirius, Vega, Regulus, &c., which, although generally called white stars, have in reality a bluish tinge. Their spectrum is characterised by four absorption lines, representing hydrogen. The stars of the second type are Capella, Pollux, Arcturus, &c. The spectrum of these stars exactly resembles the solar spectrum. The sun may, therefore, be regarded as a star of

this type. The stars of the third type are characterised by a spectrum having absorption lines similar to those of the second type; but, furthermore, containing a great number of nebulous bands forming a sort of colonnade superposed upon the spectrum. The stars of the fourth type consist of an inconsiderable number of small blood-red stars. Their spectrum exhibits three fundamental bands, red, green, and blue.

The observations of nebulæ by Sir John Herschel, Lord Rosse, Mr. Lassell, and other modern astronomers, constitute an important extension of the labours of the elder Herschel in this branch of astronomy, but no definite light has yet been thrown upon the question of the resolvability of such objects. The results of spectroscopic observation would seem to indicate that, in many instances, their substance is mainly gaseous.

III.—*Comparison of the Vital Statistics of the Different Districts of Glasgow, for the Two Years,* 1869 *and* 1870. By Mr. Alexander Scott.

Read before the Section of Sanitary and Social Economy, 27th November, 1871.

They will be happy times indeed, when it will be necessary to offer any apology for bringing the subject of Glasgow Mortality before the Sanitary Section of the Glasgow Philosophical Society. Of itself, it is a subject of the most pressing and vital importance to every citizen of Glasgow; and if there is one subject that can more appropriately engage our attention, as a Section, than another, it is the question—What can we learn as to the nature and the haunts of those diseases which produce the great excess of mortality which distinguishes our city?

A great waste of human life is undoubtedly going on in our midst, and we are making almost frantic efforts, in one way and another, to arrest it. The city is committed to gigantic undertakings, costing hundreds of thousands of pounds, and is contemplating others equally gigantic and expensive. Yet with all this, the most conflicting opinions prevail as to the causes of our great mortality; and a great many random assertions, or partial and misleading statements, as to its distribution, pass current for

established facts. Among these may be mentioned the very general impression which prevails, that the highness of our death-rate is due principally, if not wholly, to epidemic diseases, and that these scourges assume the most extraordinary proportions in two of our districts—the Central and High Church—the bad parts of which are supposed to suffer so frightfully from epidemics as to raise the death-rates of the two districts themselves about 50 per cent. above the others, and thus actually to raise the general average for the city by 3 or 4 per annum to 1,000 of the population.

One of the most amusing illustrations of the sensational kind of statement not uncommon, is the one whose circulation was so largely extended by the President of the Social Science Congress. In his inaugural address a few weeks ago, he repeated the following extraordinary statement:—"As to dwellings alone, how suggestive are the statistics of large cities. For example, in the better parts of Glasgow the inhabitants only average 34 per acre, and in these parts the annual death-rate is 5 per 1,000. In the squalid parts (not the worst) the average is 328 persons per acre, and the death-rate 34 per 1,000; that is, 29 persons per 1,000 die annually from *mere difference of habitation.*" His authority for what he well describes as these "startling figures" is a paper published in Glasgow, by the Rev. Mr. Kirk. Mr. Kirk's authority, I presume, was the late Dr. Strang's Annual Report for 1861 (see pp. 20 and 21). But surely a moment's consideration might have prevented either Mr. Kirk or Sir John Pakington from drawing, from Dr. Strang's figures, a conclusion that would have profoundly astonished their intelligent compiler. It is true that the Report referred to states that in a certain very limited and wealthy portion of the city, the death-rate for one year was only at the rate of 5 per 1,000 of population. But if this had been a normal case it would of course have followed that the inhabitants of this highly favoured district must have lived on an average to the ripe age of 200 years! It should be remembered that, as a rule, the people of this particular part of the West End only live about half of the year in town, and, of course, deaths occurring elsewhere are registered where they *occur*. Further, more than a third of the entire number are domestic servants, who are not allowed to die in their employers' houses, but, when anything is wrong with them, are sent to the Infirmary, or home to their friends. And again, when any of the wealthy residents themselves are ill, they are generally sent out of town for change of air, and even they are not allowed to add to the death-rate of the Crescents.

Notwithstanding the erroneous character of many of these ideas, they are apt, by-and-by, if uncontradicted, to be believed, and in some cases acted on; and it thus becomes all the more necessary that we should sift this subject as thoroughly and fairly as possible, endeavouring to throw away, or keep in abeyance, all theories and all pre-conceived notions whatsoever, and, by a purely inductive process, to acquire all the information that we possibly can in reference to the *actual state of the case.*

The present time is favourable to such an attempt. Mr. William West Watson's excellent Report on the Glasgow census, taken in April last, enables us to correct the estimates of the population in the various districts, and so reduces to comparative certainty many things that were mere matters of speculation before.

I start with the supposition that our death-rate is unsatisfactory. How far it is so, as compared with the death-rates of other towns, it would be labour lost to attempt to determine until the details of the general census are before us, when I trust this particular aspect of the subject will receive, from some other and abler hands, the exhaustive treatment which its importance requires. My present object is more to compare district with district within the city, than to compare Glasgow with other towns.

In order to do this as fairly as possible, the Registrar-General's returns for these districts have first to be corrected. And this is no easy matter. According to a rule in our registration system—a rule which ought, surely, to be altered—every death occurring in an hospital is registered in the district in which the hospital is placed, and not in that from which the patient was brought. Thus, districts happening to contain hospitals are burdened with a mass of deaths which do not belong to them at all; while the districts from which the invalids were removed have their bills proportionately reduced. Now, with the assistance of our obliging President, and through the kindness of gentlemen connected with the various important hospitals in the city, I have been able to ascertain the number of deaths so registered; and also, with sufficient accuracy, how many of these deaths were due to fevers, and how many to other zymotic causes. These I have, in the first place, deducted entirely from the particular districts in which they were registered. They are as follows (the two years being added together):—*

* It should be stated that as the Barony Poorhouse, &c., are situated beyond the city boundaries, the deaths occurring there are never registered in Glasgow at all.

DEATHS IN HOSPITALS—TWO YEARS, 1869–70.

Institution.	Fevers.	Other Zymotics.	General Causes.	Total.	District of Registration.
Royal Infirmary, ...	429	7	715	1,151	High Church.
Town's Hospital, ...	...	90	1,185	1,275	Central.
Fever Hospital, Parliamentary Road, ...	482	...	...	482	Ditto.
Fever Hospital, Belvidere, .	6	...	...	6	Bridgeton.
Govan Parish Poor's House, .	53	...	317	370	Tradeston.
	970	97	2,217	3,284	

Next comes the question of relegating these deaths to the districts from which the cases were drawn. This, the information I have been able to obtain, does not enable me in all cases to do. So far as the Royal Infirmary is concerned, the difficulty is not great. Its admirable Reports give full particulars of the fever and small-pox deaths occurring in that institution, specifying the districts from which the patients came, along with the results of treatment, and these I have distributed accordingly. In regard to the other causes of death, the Reports give only the number of cases *admitted* from each district, without stating how many from each, respectively, proved fatal. But no serious error can arise from assuming the deaths to be in proportion to the cases admitted, and I have distributed them on that principle. Besides these cases from the different districts, there remain a large number of cases (fully one-fourth of the whole) which were brought from beyond the registration boundaries—some of them from very considerable distances—and these are consequently deducted from our returns altogether. In reference to the deaths in the other hospitals, I have failed to obtain any information such as would enable me to relegate them to the districts from which the cases really came. And, as the plan which some have proposed, of distributing them simply in proportion to the population of the districts, could do no good, and might be misleading, I think it less objectionable, on the whole, to eliminate them from the returns altogether, and confine our attention to cases treated *at home*, together with those in the *Royal Infirmary*.

It can hardly be necessary to remark here that, as we have thus deducted from the deaths registered in the city a large number known to belong to places beyond the boundaries, and a still greater number whose proper domiciliary districts I have been unable to obtain—and many of whom, indeed, were houseless—it follows that

we will necessarily obtain a smaller number of deaths, and a lower aggregate death-rate for the city, than ought to appear in ordinary returns; and this fact must be borne in mind in comparing the calculations which follow with any others.

It will be well to state here, also, the mode adopted in estimating the mean population of the city itself, and of the various districts of the city, for the period of two years, 1869-70, now under review. The census of 1871 has been compared with the census of 1861; and of the change during the ten years, seven-eighths are assumed to have taken place by the middle of the period in question. That is a pretty close approximation to the proportion of the decade; and if the fraction be a little either too large or too small, the error must be very trifling. The results are the following:—

ESTIMATED POPULATION—MEAN FOR 1869-70.

Central, . . .	56,309
High Church, . . .	57,176
Bridgeton, . . .	58,735
Calton, . . .	37,982
Clyde, . . .	26,271
Blythswood, . . .	32,849
Milton, . . .	44,754
Anderston, . . .	51,001
Tradeston, . . .	47,609
Hutchesontown, . .	54,172
Total, .	466,858

I would have preferred, however, if it had been possible, to base the estimate of the population in each district on the number of inhabited houses in it, multiplied by the average number of persons to a house, in that particular district at the preceding census. It is easy to suppose that the growth of individual districts is less regular than that of separate towns; for many local circumstances serve to divert the stream of population hither and thither to a very considerable extent. On the one hand, the mere throwing into the market of a quantity of ground, eligible for building purposes, but which had been long kept back; or, on the other, the exhaustion of suitable ground for these purposes, in any particular place, will have an appreciable temporary influence in promoting or retarding the progress of population in any one direction. Again, the erection of large manufactories in one place, or the shutting up of important public works in another, must also have a considerable effect. Thus, in particular localities, we have building epochs, and now-a-days we have also periods of demolition, so that the popula-

tion of particular districts cannot be expected to progress with even the comparative steadiness of independent towns. Mr. Watson finds it possible, from time to time, to make relatively a very correct estimate of the *whole* population of the city, on the basis of the inhabited houses appearing on the rent-roll. And if this plan commends itself over any other in respect to the whole city, it ought, for the reasons alluded to, to be of still greater value when applied to individual districts. So far as I can see, there should be no more difficulty in giving, every year, a very reliable estimate, on this basis, of the population of *each district*, than in giving one for the whole city; and that would remove one of the discouragements hitherto held out whenever any one suggested the desirability of publishing official sanitary returns of an exhaustive character for the various districts.

There are various ways of judging of the sanitary character of a district. I shall speak of two. The first is one which I think is seldom made use of to such an extent as it ought to be. It is—judging the character of the district by the CHARACTER of the deaths. A great deal may be learned by finding to what kinds of disease particular districts are specially liable.

In all inquiries in this direction, great importance is attached to one class of diseases—the zymotic, or epidemic and contagious class. These being considered as preventible, their presence indicates neglect of the more simple and obvious sanitary laws, and the degree of their prevalence, in a particular place, is regarded as a sort of index of the sanitary conditions which there exist.

The only causes of death for which the Registrar-General gives district details are contained in this class. He singles out the principal members of the group as follows :—Fevers, small-pox, measles, scarlatina, diphtheria, hooping-cough, diarrhœa and cholera. And the deaths from these causes, and from these only, he reports separately for every district. For our present purpose we shall divide them into two groups. First, the fevers. These form a very distinctly marked class by themselves, and are pretty equally spread over all ages. Second, the other seven diseases named. These, with the exception of diarrhœa and of cholera (happily in the meantime unimportant), are almost wholly confined to children. For brevity's sake they will be hereafter referred to simply as our second group.

I have accordingly prepared a table showing the degree to which these epidemics prevail in the different districts. In every 1,000 deaths the number due to these causes is as follows:—

ACTUAL NUMBER OF ZYMOTIC DEATHS IN 1,000, FROM ALL CAUSES.

	Fevers.	Second Group.	Total.
Anderston, . .	63·19	191·43	254·62
Tradeston, . .	44·77	196·12	240·89
Hutchesontown,	52·84	179·54	232·38
Milton, . . .	61·96	150·10	212·06
Calton, . . .	66·64	143·14	209·78
Bridgeton, . .	58·32	150·06	208·38
High Church, .	48·12	150·98	199·10
Clyde, ...	59·45	139·60	199·05
Blythswood, . .	45·42	138·43	183·85
Central, . . .	37·87	121·80	159·67
Whole City, .	53·61	156·84	210·45

This I have endeavoured to render clear to the eye by diagram No. I., in which there is a column for each district. These columns are all one height, representing 1,000 deaths in every case. The proportions of the columns made up by the causes we are speaking of are shown by the shaded portions; the dark shading representing the fevers, and the other representing our second group.

It will be observed that, taking the whole of what I may call this black list, Anderston occupies the worst position, with Tradeston and Hutchesontown for second and third; and that, contrary to what is certainly the general impression, the Central district occupies the very best position, with Blythswood second, Clyde third, and High Church fourth. Over the whole city, fully 21 per cent. of the total deaths are attributed to these causes; in Anderston, about 25½ per cent. are so described; while in Central district only 16 per cent. (being 2 per cent. less than in Blythswood) come under this head.

But all this may be very misleading. We know that a very large proportion of the class of deaths we are speaking of occurs among children. Indeed the risk of death from *all causes taken together* is much greater among children than among adults; but this is more especially the case in reference to the seven zymotic diseases which constitute our second group. To show the relative risks at different periods of life, as made up from the actual deaths in Glasgow during the two years under investigation, I have tabulated them under the ages given by the Registrar-General, showing opposite each stage of life the per-centage it forms of the whole population, and the per-centage it contributes of the deaths—from all causes taken together; from fevers separately; and also from our second group separately:—

PER-CENTAGE OF PERSONS LIVING, AND OF DEATHS OCCURRING, AT DIFFERENT AGES.

Ages.	Per-Centage of Population.	Per-Centage of Deaths Registered.		
		All Causes.	Fevers.	Second Group.
Under 5, . .	13·702	46·141	14·486	80·262
5 to 20, . .	31·148	11·324	18·928	15·079
20 to 60, . .	50·152	30·025	59·826	3·030
60 and upwards,	4·998	12·510	6·760	1·629
	100·000	100·000	100·000	100·000

And from these data, when the average risk over all ages is called 1, we estimate the risk at each separate period of life to be as follows:—

DEGREE OF RISK AT VARIOUS AGES, WHEN THE AVERAGE RISK AT ALL AGES IS CALLED 1.

Ages.	From all Causes.	From Fevers.	From Second Group.
Under 5, . . .	3·367	1·057	5·858
5 to 20, . . .	·364	·608	·484
20 to 60, . . .	·599	1·193	·060
60 and upwards, .	2·503	1.352	·326
All Ages, . .	1·000	1·000	1·000

It will be noticed that under five years the risk is considerably more than three times as great as the average over all ages, when we consider all causes of death together; as compared with the risk at the period 20-60, it is nearly six times as great. In regard to fevers the risks are not very unequal. But in respect to our second group, the infantile risk is nearly six times that of the average over all ages, and actually little short of a hundred times as great as the risk from twenty to sixty years.

Bearing in mind, then, that in one of the groups—and that the larger one—that we are dealing with, the risk among children is so great—being almost confined to children, indeed—it becomes of great importance to ascertain what proportion of the population in each district is under five years of age; what per-centage between

five years and twenty years; what between twenty and sixty; and what above sixty; and to make up from these data the proper allowance to make for the peculiarities of each—or in other words, to determine how far, from the mere composition of the population as to age, we should EXPECT each individual district to vary from the general average for the whole.

For this purpose I have calculated the per-centage composition of the population in each district, as shown by the census of 1861, compared the results with those of the census in 1871, and in striking the mean as at the middle of our two-year period, have gone on the same principle as when estimating the number of the population. The following are the results:—

PER-CENTAGE OF PERSONS LIVING AT DIFFERENT AGES, IN EACH DISTRICT.

	Under 5 years.	5 to 20.	20 to 60.	60 and upwards.	Total.
Central, . .	13·369	30·362	50·838	5·431	100·000
High Church,	14·085	32·527	48·522	4·866	100·000
Bridgeton, .	14·429	33·544	47·193	4·834	100·000
Calton, . .	13·528	30·683	49·912	5·877	100·000
Clyde, . . .	12·173	29·824	52·894	5·109	100·000
Blythswood, .	10·470	29·347	54·093	6·090	100·000
Milton, . .	14·809	31·383	49·579	4·229	100·000
Anderston, .	13·757	29·960	51·606	4·677	100·000
Tradeston, .	13·639	31·035	50·320	5·006	100·000
Hutchesontown,	14·778	31·025	49·727	4·470	100·000
Whole City, . .	13·702	31·148	50·152	4·998	100·000

It may be mentioned in passing, that while this method of comparing the mortality of different districts is altogether independent of the magnitude of the population at risk, even the allowances it requires to be made are dependent on what changes much more slowly, and within much narrower limits—namely, its per-centage composition. Thus we have a district increasing in population by fully one-third in ten years; but during that period the greatest change in the number of children, in any one instance, is from 12·9 per cent. to 12·1 per cent.—a difference comparatively minute. Indeed, in many cases, this is the only system of comparison available—the aggregate number of the population being so uncertain.

Taking, then, the per-centage composition of the population of any district, and knowing the respective degrees of risk in each division of ages, we are able to compute the expectation of mortality —the actual risk-rate—for that district; either in respect to all

causes combined, or, separately, to fevers, or to the second group in our black list. And it is fairer, of course, to base it solely on Glasgow data, than on those of any other or larger area; for it might otherwise have been objected that Glasgow may vary, in respect to relative risks at different ages, from other places—for instance, might be more or less healthy for children, as compared with adults, than the average of other towns.

To some it may seem hardly credible that the difference in mortality at different ages can be so extreme. But we have already seen that those in the period of life from twenty to sixty years of age compose almost exactly one-half of the population, but contribute only 30 per cent. of the whole deaths; while the children under five years, although little more than one-fourth as numerous, contribute 46 per cent. And, coming again to our second group of the zymotics, we have seen that children under five contribute 80 per cent. of the deaths under that head; and those who have just passed into the next stage older contribute the most of the remainder. Indeed, it is only here that the risk of the second division as to ages—those from five to twenty—is not the lowest of any. In the aggregate it has by far the lowest risk of any of the four periods of life to which our tables refer.

In connection, however, with this subject, I have thought it might be interesting to give in some way, to the eye, a picture or condensed view of the actual composition of the population of the city, as shown in the late census, and have attempted to do this by means of diagram No. II.

The height of the plain curve represents the number of males living at any age, as compared with other ages, from 1,000 new born infants, down to the thin, thin remnant at ninety years of age. The numbers beyond are too insignificant to be visible on the diagram.

The terrible suddenness with which the curve descends in the earliest stages of life, too fully corroborates our estimate of the infantile risk. A striking feature of the diagram is the way in which the curve ceases to fall, and actually tends rather to rise, at the period of youth and early manhood. This is accounted for by the influx of young persons, in the full bloom of health and strength, from the country. It must be borne in mind that fully one-third of the increase of our population is derived from without. Of course we have some of this increment at all ages; but a very large proportion must have passed the period of greatest risk, and come to us with the reasonable expectation of a fair lifetime. Were it not for

this circumstance, it is depressing to conjecture what our death-rate might be.

Another source of difference in the expectation of mortality, is the difference in the proportion of males to females. That there is a greater risk among males than among females, is a well known fact. In Glasgow, during these two years, it has been nearly 8 per cent. higher; and even that is a considerably smaller difference than the Registrar-General gave for all Scotland, in his report on the census of 1861. The effect of the possible difference in the proportions of males and females drafted into the city from other places, makes it unsafe to attempt to determine how this difference of risk varies at different ages. But in the diagram, along with the plain curve which refers to the males, I have represented, by a dotted line, the curve which would outline a corresponding view of the females living at different ages. It begins, not with 1,000 new born infants, but with 956, which is about the proportion of females born to 1,000 males, during the year preceding the census. The shape of this curve; the way in which it soon crosses and rises above the plain one; and more especially its much greater height, relatively to the other, in the later periods of life, confirm in a general way the greater vitality of females.

It is obvious that this must have a bearing on the mortality of districts in which the proportion of males to females differs. But on calculation I find that the correction required would in no case be very considerable. For instance, Blythswood has the lowest percentage of males; and all the difference our correction would make, would be to depress our expectation of its aggregate death-rate per 1,000 of the population by about *half-a-tenth*. These corrections, therefore, I have not thought it worth while to make.

Reverting, now, to our statement of the prevalence of zymotics in the various districts, as shown by the proportion zymotic deaths bear to the total number of deaths, we proceed to apply the corrections for difference of populations as to ages. Over the whole city, 53·61 out of every 1,000 deaths are due to fevers, and 156·84 to our second group of zymotics. Adding to these averages, or deducting from them, according to the risk from the actual mixture of population in each particular district, we obtain results tabulated below. These results are also shown on diagram No. I., by means of zig-zag lines, which we may call "expectation lines." The lower one represents the height it is reasonable to expect fevers to attain, in the columns; and the upper one, the expectation height for fevers and second group combined.

EXPECTATION OF DEATHS FROM PRINCIPAL ZYMOTICS IN 1,000 FROM ALL CAUSES.

	Fevers.	Second Group.	Total from these Causes.
Central, ...	53·86	153·27	207·13
High Church, . . .	52·87	160·34	213·21
Bridgeton, ...	52·19	163·06	215·25
Calton, ...	53·15	153·25	206·40
Clyde, ...	56·22	147·51	203·73
Blythswood, ...	58·25	135·37	193·62
Milton, ...	52·57	164·14	216·71
Anderston, ...	54·04	156·64	210·68
Tradeston, ...	53·72	156·33	210·05
Hutchesontown, . .	52·47	162·99	215·46
The City Average being	53·61	156·84	210·45

Then comparing these "expectation" rates with the actual rates already shown, we get the following statement of differences: showing where the actual rate is over the expectation, and where it is under it:—

	Fevers.		Second Group.		These Causes together.	
	Over.	Under.	Over.	Under.		
Anderston, . .	9·15	...	34·79	...	43·94	over.
Tradeston, . .	...	8·95	39·79	...	30·84	,,
Hutchesontown,	·37	...	16·55	...	16·92	,,
Calton, . . .	13·49	...	...	10·11	3·38	,,
Milton, . . .	9·39	...	...	14·04	4·65	under.
Clyde, ...	3·23	...	...	7·91	4·68	,,
Bridgeton, . .	6·13	...	...	13·00	6·87	,,
Blythswood, . .	...	12·83	3·06	...	9·77	,,
High Church, .	...	4·75	...	9·36	14·11	,,
Central, . . .	...	15·99	...	31·47	47·46	,,

On examination into these differences between the expectation and the actual, we now find some districts have changed places in regard to excellence. Anderston, with an excessive per-centage, both in fevers and in the second group, still occupies the worst position, having actually attributable to these causes 44 more out of every 1,000 deaths than the average. As before, Tradeston and Hutchesontown follow next in order; but next to these comes now Calton, which has exchanged places with Milton. The Central still occupies the place of honour, with High Church promoted to the second place, leaving Blythswood third. It may be said, and per-

haps fairly, that this estimate is too favourable to the Central and High Church districts, because, of the deaths occurring in the Fever Hospitals and the Town's Hospital (and which we have excluded from our calculations), a very large number are probably drawn from these districts. But if we had retained them, and—regarding the Central and High Church as one great district—had charged them EXCLUSIVELY to it, all these deaths put together would not have raised its total zymotic per-centage to the calculated expectation point. The fever ratio would have been high, but the per-centage from our second group would have been sufficiently low to do more than cover the difference, leaving the per-centage from these causes together still under the average.

Now, it is quite possible that when we come to the death-rate per 1,000 of the population, we may find a district occupying the very worst position, that stands in a very respectable place here. But in that case we will at once see that the excess of its mortality is due, not to zymotic causes, but principally or wholly to other causes; and if we are to take more than a very partial view of the case, we must be equally careful to keep BOTH facts with equal distinctness before the mind. The very first element in any rational scheme of a remedial character, is that it be based on an accurate knowledge of what is really wrong. It will not do to rest contented with knowing that we have people ill and dying; if we want to cure them we must first know what is their disease. Otherwise we shall too closely resemble the two tailors who were inordinately conceited about their skill. The one boasted that if he saw a man going through a close, he could make a coat to fit him. "Pooh!" rejoined the other, "that's nothing; I could make a coat to fit him, if I saw the close he went through." We shall be equally wise if we think that, by merely inspecting the closes, we can tell what diseases are revelling up stairs. I speak with diffidence; but is it not barely possible that hitherto, in dealing with our high death-rate, our attention has been just a little too completely restricted to zymotic diseases? My present object is, as far as possible, to exhibit the actual statistics of the city, leaving it to others to found upon them, so far as they may consider them reliable, whatever conclusions they please; but I cannot pass from this part of the subject without drawing attention to the bearing it must have upon the City Improvement scheme, and the projected purification of the Clyde.

Another mode of judging of the sanitary character of a district is by the NUMBER of the deaths, relatively to a given number of population. This, of course, is the ordinary way, and forms the basis of

the Registrar General's published death-rates. The average aggregate death-rate for the city for these two years is 29·089 per annum for each 1,000 at risk, always bearing in mind that this is exclusive of some of the deaths in hospitals. The rate for each district separately is stated in the following table, accompanied by the expectation rate calculated on the principle already explained, and without reference to which the simple aggregate rates are almost valueless. The table shows how far the actual rate is over or under "expectation":—

ACTUAL AND EXPECTATION DEATH-RATE PER 1,000 OF THE POPULATION—SHOWING HOW FAR THE ACTUAL IS OVER OR UNDER THE EXPECTATION.

	Actual.	Expectation.	Difference.
Clyde, ...	35·857	28·017	7·840 over.
Calton, ...	33·384	29·474	3·910 ,,
High Church, . .	33·073	29·237	3·836 ,,
Bridgeton, . . .	30·067	29·427	·640 ,,
Hutchesontown, .	29·868	29·678	·190 ,,
Central, ...	28·139	29·121	·982 under.
Anderston, . . .	26·529	29·044	2·515 ,,
Tradeston, . . .	26·508	29·057	2·549 ,,
Milton, ...	26·869	29·545	2·676 ,,
Blythswood, . . .	21·112	27·221	6·109 ,,
Whole City, ...	29·089	29·089	.....

The same thing is represented in diagram No. III., where, as before, there is a column for each district. The height of the column is proportioned to the actual aggregate death-rate, and the expectation rate is shown by a dotted line. In this instance, an attempt is made to convey an idea of the numerical importance of the different districts, the breadth of each column being proportioned to the magnitude of the population which it represents.

Here it will be seen that the Clyde district, although one containing relatively a small proportion of children, and which therefore should have a low death-rate, in reality possesses by far the highest. After it come Calton and High Church. Blythswood is by far the best, with Milton, Tradeston, and Anderston pretty equally contesting for the second place; after which comes the Central, not (as is so often supposed) away up about 40, but with a rate of 28·139 —very considerably below the expectation.

Combining the results of the two lines of inquiry, it thus appears that by far the highest death-rate is in Clyde district; but that this cannot be attributed to epidemic diseases, for in Clyde district less

than the average proportion of the deaths registered are from these causes—proving that zymotics are not so much aggravated as general causes of death. Calton comes next, though its epidemic proportion is not much above the corrected or "expectation" average. High Church, instead of being far a-head, is third; and Central comes away down sixth, and actually considerably below the average. In regard to the proportion of epidemics to other diseases, the Central occupies by far the best position of any, and High Church is next. Blythswood, although it has by far the lowest death-rate, is behind these two districts in this respect. Tradeston and Anderston have low aggregate rates; but Anderston has the very highest proportion, and Tradeston the next, of epidemics, as compared with other diseases. It seems impossible to resist the conclusion that instead of epidemic diseases being the sole, or even the principal cause of our high death-rate, there is no very close connection between the two things.

It is interesting to note here, how the density of the population is related to the foregoing tables—whether it corresponds more closely to the aggregate death-rates of the districts, or to the liability to zymotics as compared with general causes of death. I have therefore given a table of the density of the population, showing the districts separately, the city as a whole, and the other seven large towns in Scotland :—

DENSITY OF POPULATION.

	Persons to an Acre.		Persons to a Room.
	In 1861.	In 1871.	In 1861.
Bridgeton, ...	47	62	2·68
High Church, . . .	41	50	2·48
Clyde, ...	229	200	2·45
Calton, ...	178	186	2·33
Hutchesontown, . .	99	125	2·26
Central, ...	133	153	2·02
Milton, ...	86	120	2·01
Tradeston, ...	92	117	1·81
Anderston, ...	59	75	1·72
Blythswood, . . .	109	127	1·25
Whole City, .	78	94	2·05
Paisley, ...	.....	.....	2·1
Dundee, ...	.....	.....	2·0
Greenock, ...	.....	.....	1·7
Leith, ...	.....	.....	1·6
Aberdeen, ...	.....	.....	1·5
Edinburgh, ...	.....	.....	1·4
Perth, ...	.....	.....	1·3

It is usual to indicate the density by the number of persons to an acre. The utter delusiveness of this method will be seen by a glance at the table. For instance, in April last, High Church district, one of the most overcrowded in the city, had only 50 persons to the acre, while Blythswood, the best district in the city, had 127. No system which does not allow for large unbuilt spaces, extending perhaps far into the country, can give data of the slightest use. But the real question is not so much the number of people to an acre—the question of paramount importance is, How many people are stowed into a ROOM, to have its limited supply of oxygen shared among them? In the table this is shown for the year 1861. Unfortunately we have no information for 1871. Indeed it is doubtful whether this most important matter was not overlooked in the census this year.

It will be seen that in 1861 the largest number of persons to a room was in Bridgeton; next came High Church, Clyde, and Calton. These four districts, although not in this order, also stand at the top of the aggregate death-rate table. Then, if Anderston and Milton, which are nearly equal in respect to aggregate death-rate, were to exchange places, all the districts which follow the four already named would occur in precisely the same order in both tables. No such close relation can be traced between the density and the relation of zymotic deaths to other deaths. Neither does the density correspond with the absolute zymotic deaths to the 1,000 of the population, or to the fever portion of them taken separately.

In reference to the somewhat extreme difference in death-rate between Blythswood and any of the other districts, it should not be forgotten that Blythswood is, to some degree, affected by all the peculiarities already pointed out as explaining the secret of the highly favoured district referred to by Sir John Pakington, where the death-rate was only five. If we knew precisely how much to allow for the extent to which it is so affected, it is quite possible that a large portion of its prestige might turn out to be fictitious. Perhaps we are too apt to forget, on the other hand, that a great deal of the excessive mortality in the *poorest* localities may be due to the circumstances of the people, rather than to the condition of the districts. When well, many people must find it impossible to get sufficient food; and without an organized system of inspection, under the charge of a properly qualified analyst—a thing that seems to be very much needed—we have no guarantee that what they do get is not often unwholesome. Then the want of sufficient clothing; the inability, in case of sickness, to procure adequate medical assist-

ance; or, even if advice and medicine be obtained gratuitously, the inability to pay for the nourishing diet and generous cordials which are so often necessary to give the purely medical treatment a fair chance; and besides all that, the tyranny of toil, which leaves little time for nursing, and the ignorance which must detract wofully from the value of such nursing as is given—these are all things tending to raise the death-rates of the poorest districts, and things which will not be remedied by any city improvements whatsoever. Pull down hovels as you may, these people can only take their poverty, their ignorance, their misery—and their disease—elsewhere.

Returning to the mode of stating the character of a district by the death-rate per 1,000 of the population, it is obvious that to make it at all complete, it is necessary to give all the details which go to make up the aggregate rates, comparing these with the corrected average, or expectation, figures, and showing the gain or loss under each head. This I have done, so far as fevers and our second group of zymotics are concerned, the lack of district details obliging me to put all the "other causes" together. We have already seen what per-centage of the deaths in each district is made up from fevers, and how much from our second group. What that amounts to per 1,000 of the population in each case, is shown in the following table, which is an analysis, so far as it goes, of the respective rates :—

DETAILS OF THE ACTUAL DEATH-RATES PER 1,000 OF THE POPULATION.

	Fevers.	Second Group.	Other Causes.	Total.
Clyde, ...	2·132	5·005	28·720	35·857
Calton, ...	2·225	4·778	26·381	33·384
High Church, . .	1·592	4·993	26·488	33·073
Bridgeton, . . .	1·753	4·512	23·802	30·067
Hutchesontown, .	1·578	5·363	22·927	29·868
Central, ...	1·066	3·427	23·646	28·139
Anderston, . . .	1·677	5·078	19·774	26·529
Tradeston, . . .	1·187	5·199	20·122	26·508
Milton, ...	1·665	4·033	21·171	26·869
Blythswood, . .	·959	2·923	17·230	21·112
Whole City, . . .	1·559	4·563	22·967	29·089

I have also shown the same thing on the diagram (III.) by shaded portions, in the same way as before. The dark shading, at the foot of the column, represents the fevers; the next portion, shaded

lightly, represents our second group; and the upper portion, left unshaded, represents general causes. Some of the districts, with comparatively low *per-centages* of zymotics, as compared with the deaths from all causes, will be observed to have a large *absolute amount* of such deaths; the reason being obvious. Although the per-centage is low, the deaths on which it is calculated are excessive.

The expectation details are indicated by dotted lines on the diagram, and are also shown in the following table:—

DETAILS OF THE EXPECTATION DEATH-RATES PER 1,000 OF THE POPULATION.

	Fevers.	Second Group.	Other Causes.	Total.
Clyde,	1·575	4·133	22·309	28·017
Calton,	1·566	4·518	23·390	29·474
High Church,	1·545	4·688	23·004	29·237
Bridgeton,	1·535	4·799	23·093	29·427
Hutchesontown,	1·557	4·838	23·283	29·678
Central,	1·568	4·464	23·089	29·121
Anderston,	1·569	4·550	22·925	29·044
Tradeston,	1·560	4·543	22·954	29·057
Milton,	1·553	4·850	23·142	29·545
Blythswood,	1·585	3·685	21·951	27·221
The City Average being	1·559	4·563	22·967	29·089

The difference between the actual and the expectation rates is exhibited in the succeeding table, which shows how far the actual rate is over or under the expectation:—

	Fevers.		Second Group.		These Zymotics together.		Other Causes.		Total.	
	Over.	Under	Over.	Under	Over.	Under	Over.	Under.		
Clyde,	·557	...	·872	...	1·429	...	6·411	...	7·840	over.
Calton,	·659	...	·260	...	·919	...	2·991	...	3·910	,,
High Church,	·047	...	·305	...	·352	...	3·484	...	3·836	,,
Bridgeton,	·218	...	...	·287	...	·069	·709	...	·640	,,
Hutchesontown,	·021	...	·525	...	·546	...	...	·356	·190	,,
Central,	...	·502	...	1·037	...	1·539	·557	...	·982	under.
Anderston,	·108	...	·528	...	·636	...	...	3·151	2·515	,,
Tradeston,	...	·373	·656	...	·283	...	...	2·832	2·549	,,
Milton,	·112	...	...	·817	...	·705	...	1·971	2·676	,,
Blythswood,	...	·626	...	·762	...	1·388	...	4·721	6·109	,,

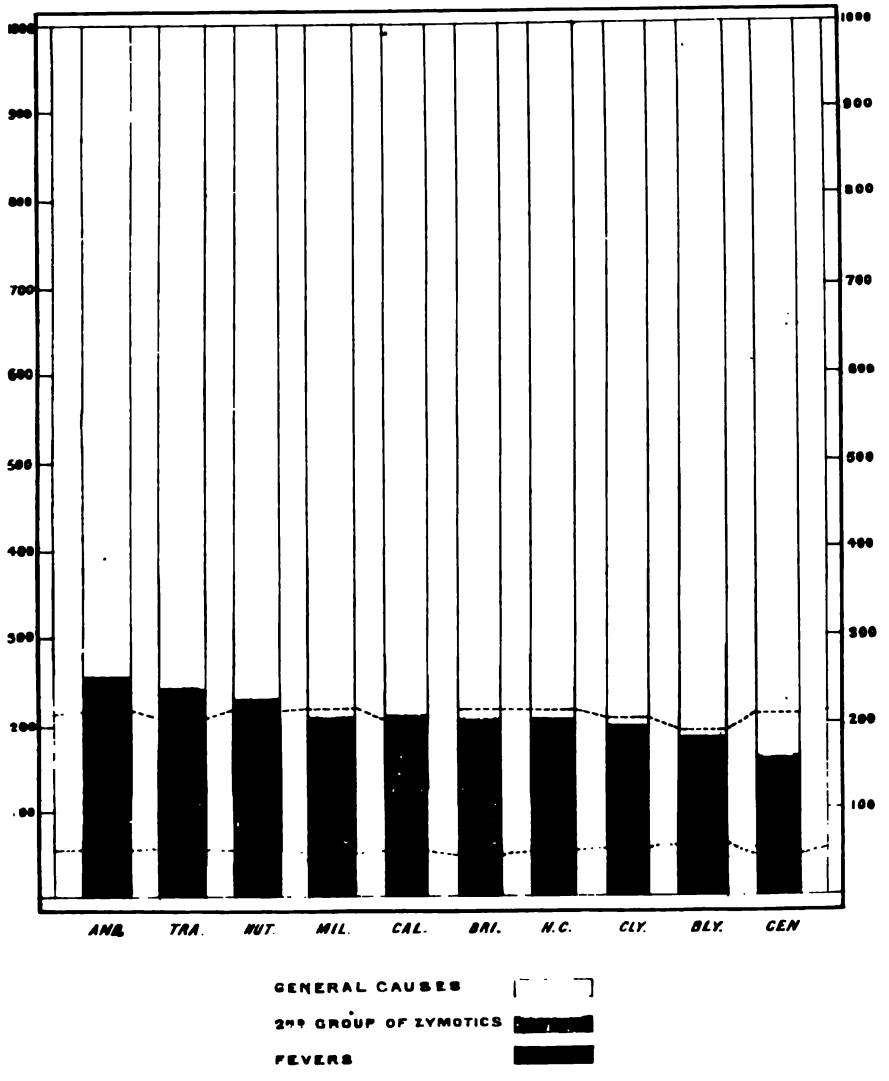
One district (Bridgeton) which has an aggregate rate above the

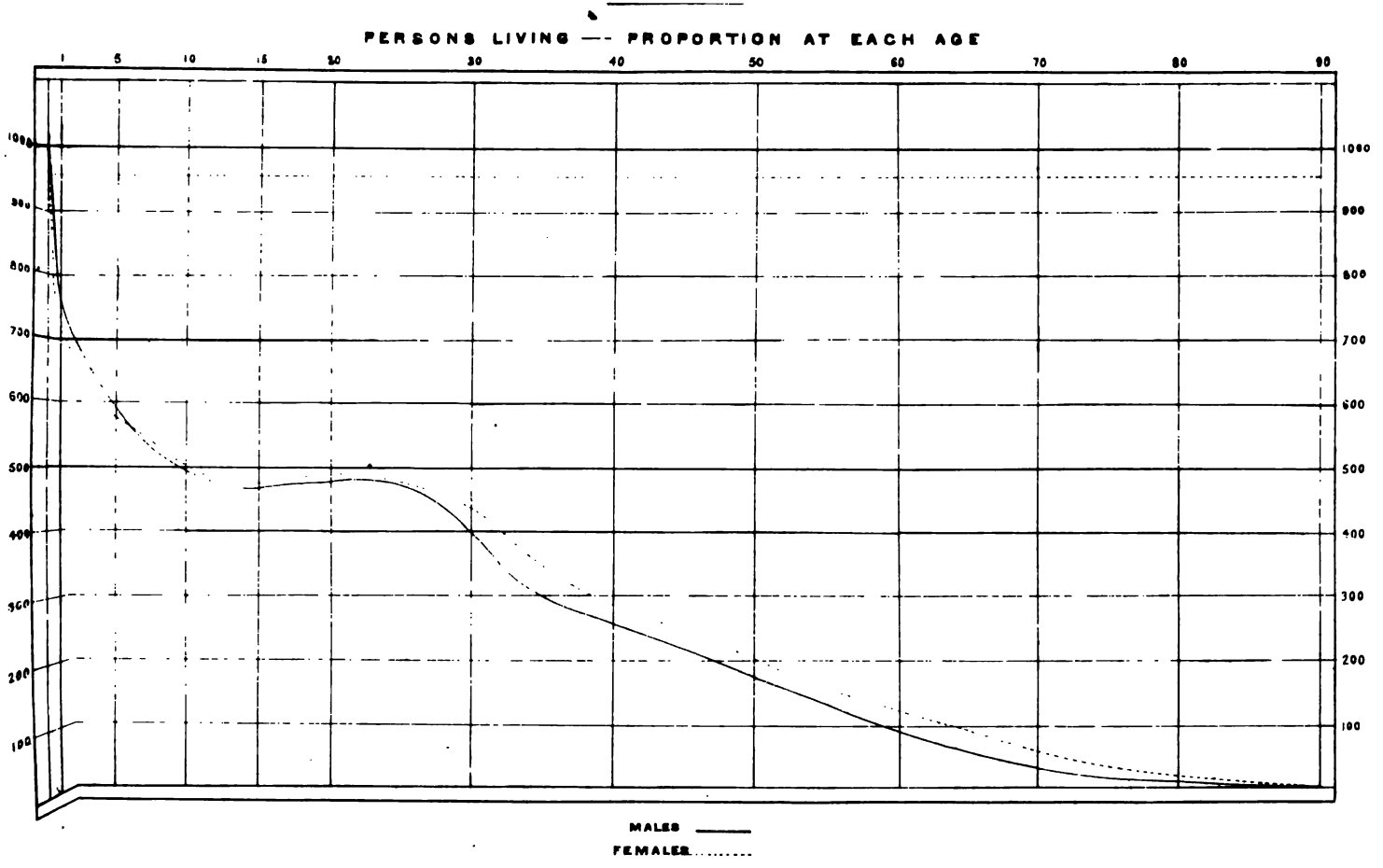
expectation, has its actual rate from zymotics below the expectation. Two others (Anderston and Tradeston) with low death-rates, have an absolute, as well as a relative, excess in zymotics—in Anderston a very serious excess. And in all the cases of high aggregate rates the excess is only to a very small extent attributable to epidemic diseases.

It would therefore be extremely interesting and useful if we could now ascertain precisely what diseases *do* account for the excess. It is particularly aggravating to look at those long columns, with their blank hideousness rising far above our corrected or expectation averages, and yet have no way of finding out what dread materials make up their ghastly proportions. But we are left in ignorance. The Registrar-General, as has been repeatedly mentioned already, gives no *district* details of any other diseases than those I have enumerated—though he gives an exhaustive analysis for each of the large towns. Dr. Gairdner has, I believe, exerted himself to induce our city authorities to make up and publish details for each district in the city, and very wisely recommended a much more minute sub-division of the city than that presented by the ten registration districts, which are far too large and mixed in character. But the economical reform mania—which is *often* penny-wise and pound-foolish—strangled the system in its infancy, and the consequence is that, in an inquiry like the present, the official returns issued from the Sanitary Chambers are of no use at all. Indeed, they condense their abstract of the deaths into five districts, and these not co-terminous with any of the Registrar-General's districts, or even with the city boundaries, so that the two systems seem as if ingeniously contrived to prevent comparisons.

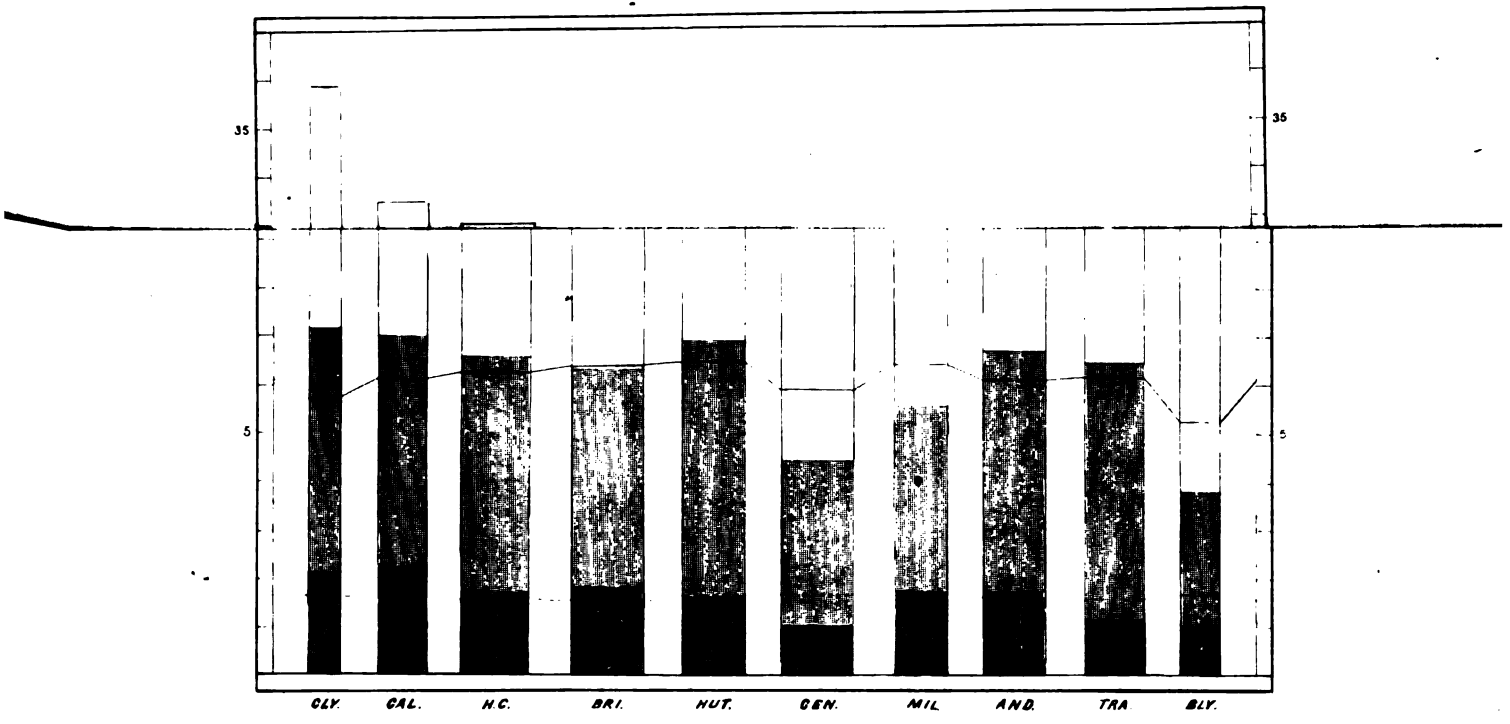
Being brought to a dead stop in our inquiry as to what diseases prevail in the particular districts where our death-rate is highest; there is but one thing more we can do before closing. It is natural to expect that, if in any particular districts, some special cause or causes should be unduly raising the rate of mortality, these are likely to betray themselves by forming a larger proportion of the total deaths in Glasgow as a whole, than they do in other towns. Now, I have calculated the proportion due to each of a considerable number of causes, in each of the eight large towns in Scotland for 1869 and 1870. I have then constructed a table, putting the Glasgow figures in one column, and the mean of the figures for Edinburgh, Leith, Dundee, Aberdeen, Perth, Paisley and Greenock in another column, showing also how far in each case Glasgow is over or under the mean of these seven towns :—

RATIO OF ZYMOTIC DEATHS TO WHOLE DEATHS.



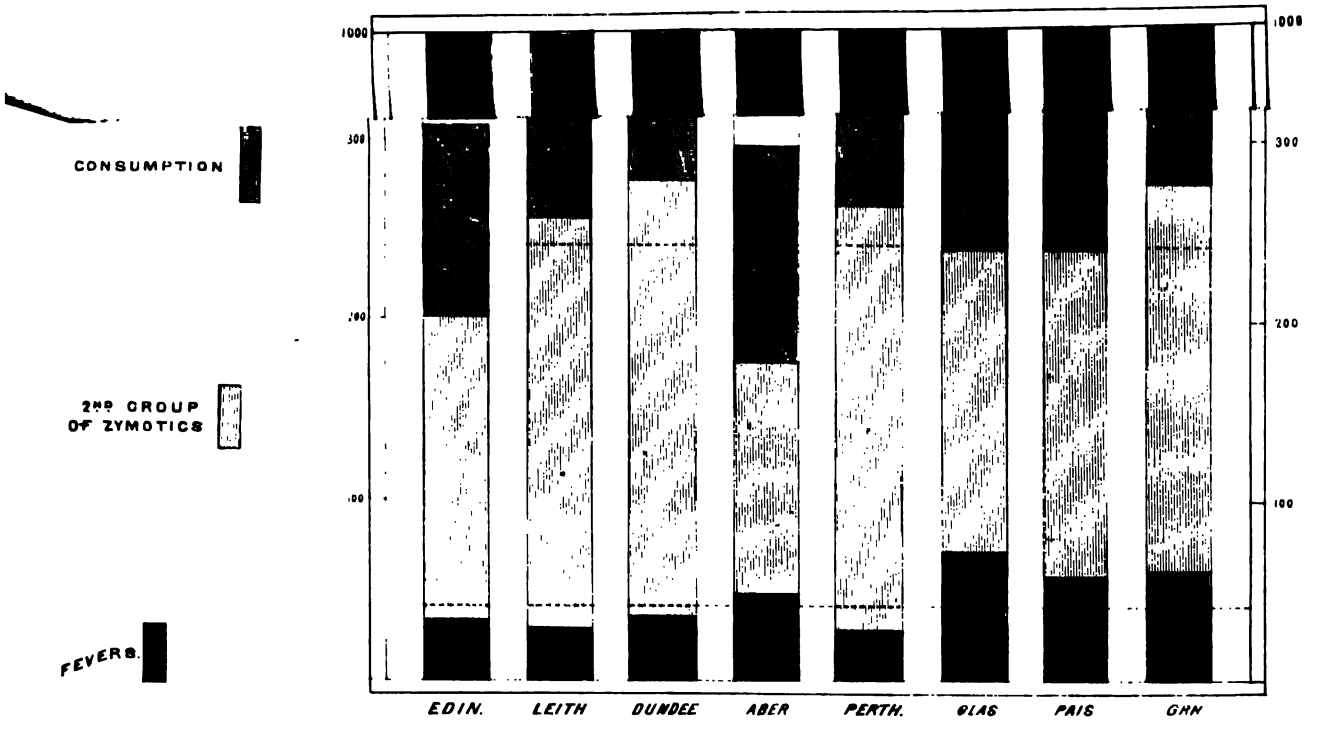

PERSONS LIVING — PROPORTION AT EACH AGE
1
5
10
15
20
30
40
50
60
70
80
90
1000
900
800
700
600
500
400
300
200
100
MALES
FEMALES

DEATH-RATES PER THOUSAND OF POPULATION

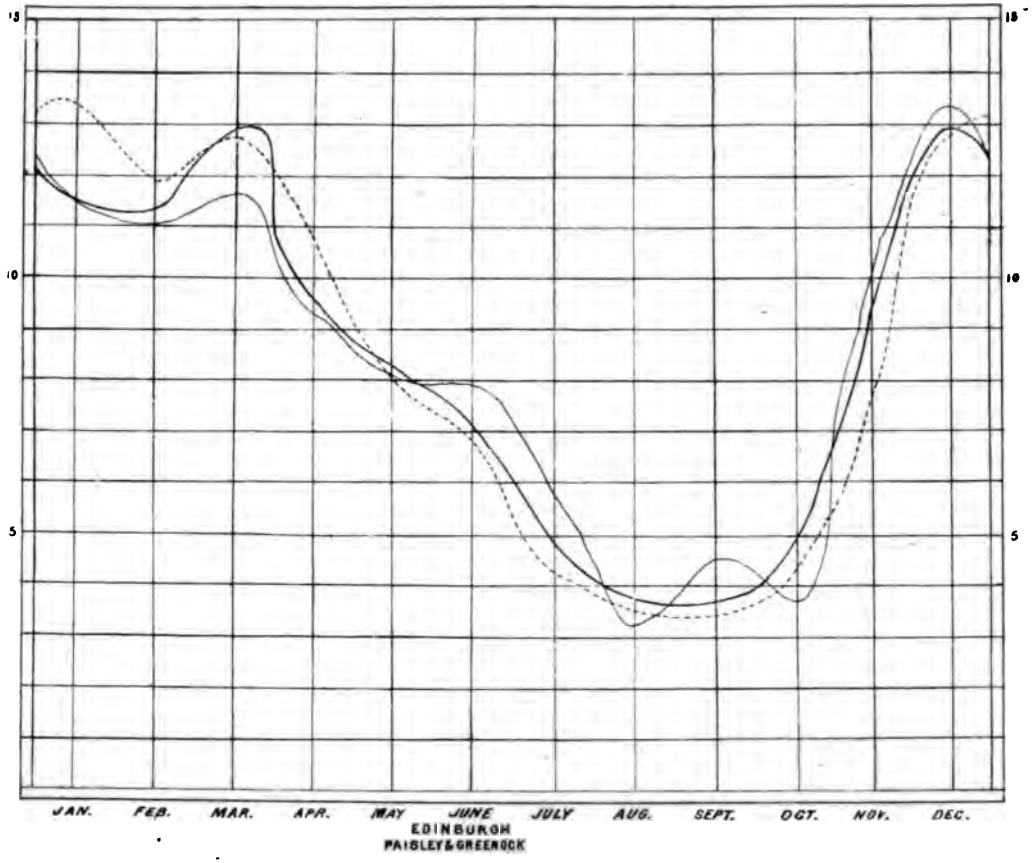


GLASGOW.

ANALYSIS OF DEATHS IN EIGHT TOWNS OF SCOTLAND



MORTALITY FROM ACUTE DISEASES OF CHEST—VARIATIONS WITH WEATHER



ANALYSIS OF 1,000 DEATHS FROM ALL CAUSES IN GLASGOW, COMPARED WITH MEAN OF SIMILAR ANALYSES FOR OTHER SEVEN TOWNS.

	Glasgow.	Mean of Seven Towns.	Glasgow.	
			Over.	Under.
Small-pox,	1·1	3·8	...	2·7
Measles,	26·2	17·0	9·2	...
Scarlatina,	46·2	76·5	...	30·3
Diphtheria,	7·0	6·5	0·5	...
Hooping-cough,	43·5	34·4	9·1	...
Diarrhœa,	22·0	29·6	...	7·6
Cholera,	1·2	1·9	...	0·7
Total from Second Group,	147·2	169·7	...	22·5
Fevers,	70·0	41·2	28·8	...
Other Zymotics,	25·6	31·8	...	6·2
All Zymotics,	242·8	242·7	0·1	...
Phthisis,	128·3	113·0	15·3	...
Hydrocephalus,	30·3	34·3	...	4·0
Disease of Heart, &c.,	29·4	49·0	...	19·6
Bronchitis, Pneumonia, and Pleurisy (combined),	205·3	141·3	64·0	...
Teething, Convulsions, and Enteritis (combined),	52·6	44·4	8·2	...
Debility from Premature Birth, &c.,	50·4	45·6	4·8	...
Age,	31·2	54·9	...	23·7
Violence, Privation, &c.,	28·5	33·5	...	5·0
Other Causes,	201·2	241·3	...	40·1
Total,	1000·0	1000·0	...	...

The epidemics which prevail will be noticed to vary very considerably, and are likely to exchange places from time to time. But it will be observed that we have here another proof that it is not in zymotics that we are to look for the secret of an excessive mortality; because these, as a class, are to blame for exactly the same number out of a thousand deaths here as elsewhere. Fevers are undoubtedly high, but other zymotics are low enough to bring down the total to the average. Consumption shows in excess in Glasgow, while, somewhat singularly, the heading "Disease of the Heart, &c.," is debited with a smaller proportion here than elsewhere. Again, old age is, unfortunately, a much rarer cause of death here than in the other towns. But the difference which eclipses all the rest in magnitude and importance is due to inflammatory diseases of the chest—bronchitis, pneumonia and pleurisy (pleurisy, it may be remarked in passing, is a mere drop in the bucket compared with the other two). Here we find deaths to the extent of 205·3, show-

ing 64 out of every 1,000 (or nearly 6½ per cent.) in excess of the average of the other places. The magnitude of this excess is realized if we consider that removing it—that is to say, simply bringing down Glasgow, in respect of inflammatory diseases of the chest to the same standard as the AVERAGE of the other seven towns—and Glasgow should be below that—would reduce a death-rate of 30 to 28, and would save nearly as many lives as if we could *exterminate fevers altogether.*

I have endeavoured to exhibit to the eye, by the diagram No. IV., the degree to which some of these causes contribute to make up 1,000 deaths in each of the towns. Fevers, as before, are represented by the darkest shading, at the foot of the columns. Next above come *all* the remaining zymotics; then phthisis. At the top, and shaded darkly, are the inflammatory diseases of the chest. The intervening space it was not necessary for our present purpose to fill up. The dotted lines across represent the average in each case, *excluding Glasgow;* and of course the first three are supposed to be measured from the bottom, while the uppermost one is measured from the top.

The excess of those inflammatory chest complaints becomes no less remarkable when we compare Glasgow with each of the other towns separately. We are far ahead even of any one of them, as may easily be seen from the diagram. Edinburgh, Leith, Dundee, and Aberdeen—as exposed East coast towns—might reasonably be expected to suffer most from these affections; and, keeping Glasgow out of sight, so they do. Perth, inland, and sheltered, suffers very much less; while Paisley and Greenock—Western towns—are also, as might have been expected, far below the East coast towns. Glasgow should naturally be expected to share in this immunity. But it is far otherwise: she has 205·3 deaths to the thousand, while the very worst rate on the East coast is 160·7.

Nor can this be accounted for by the supposition that in Glasgow there is a larger number of children relatively to adults, and that the excess of deaths is made up from their ranks. It is true that about 56 per cent. of our deaths from these causes are under five years, while the average elsewhere is only 50 per cent. But a larger proportion of our deaths, under the same head, are between twenty and sixty years, than is the case in the average of the other towns, being 25 per cent. as against 22, and larger, indeed, than in any other single town except Paisley. Where our per-centage is low is only at old ages: principally, I am afraid, from a scarcity of old people. Besides, the risks from these causes, at the different

periods of life, are not so *unequal* as in our second group of zymotics, and even there we did not find our proportion of children producing an excess—but rather the reverse.

The respective risks, it may be mentioned, are as follow:—

	From Bronchitis, Pneumonia, and Pleurisy (combined).	From Second Group.
Under 5, . . .	4·085	5·858
5 to 20, . . .	·147	·484
20 to 60, . . .	·496	·060
60 and upwards,	2·915	·326

It may also be worth while to mention, in passing, that a larger proportion of our deaths from all causes, as well as from the special causes under notice, are under five years, than is the case in the mean of the other seven towns. The following are the exact figures:—

	Glasgow.	Average of Seven Towns.
Under 5, . . .	461·41	406·55
5 to 20, . . .	113·24	111·11
20 to 60, . . .	300·25	278·40
60 and upwards,	125·10	203·94
	1000·00	1000·00

The information at my command does not enable me to give similar particulars for the different districts separately; otherwise it would have been very interesting to compare them with the percentage due to the varying proportions of children, making the calculations on the "expectation" principle already explained. We would thus have seen how far the high death-rate, in any bad district, was due to excessive mortality at particular ages.

One would like exceedingly to know whether the excessive liability to bronchitis and pneumonia is characteristic of all the districts of the city, or is confined to certain districts in particular; and if the latter, whether these are the districts we have found to have the highest aggregate death-rate, and whether this is one of

the principal causes of their unenviable pre-eminence. Perhaps if we knew all this it might help to lead to the discovery, and the remedy, of the real causes of this plague.

One other interesting question in reference to these complaints is —Does the mortality from them vary to the same extent with the weather here as elsewhere? To ascertain this I have put the deaths of the two years together, January of the one year with January of the other, February with February, and so on; then finding the average daily deaths, month by month. This I have done for Glasgow, then for Edinburgh (as an East coast town), and lastly for Paisley and Greenock combined (as representing the West country). In order to bring these results into a form in which they may be compared, the Glasgow daily averages are taken as they stand, and the daily averages in the other cases are all multiplied by the factor which brings up their respective *annual* totals to the Glasgow annual total. The Diagram No. V. is now constructed with curves based on these results. Edinburgh is represented by a thin line, Paisley and Greenock by a dotted one, and Glasgow by a bold one. It is at once seen that in Glasgow, notwithstanding our much greater liability to these complaints, the variations with the different periods of the year, are practically identical with those in other towns. It may be as well to mention that the two years taken include that memorable and fatal month—March, 1869.

So much for what the meagre returns afforded us reveal of the details of our mortality. Within the last few weeks a manuscript monthly report, of a somewhat improved and more valuable kind, has been issued under Dr. Gairdner's superintendence, and embraces one or two further causes of death, detailed for each of the ten registration districts. This, so far as it goes, is very good; but we should have very much more, and ought not to rest contented until we get each of the existing registration districts subdivided into comparatively small districts—the subdivision being guided much more by similarity of character and circumstances than by equality of numbers; and until we get the regular official publication of returns, giving for *each of these subdivisions* all, or nearly all, the details which the Registrar-General at present gives for each of the large towns of Scotland. Nothing short of this can ever give the requisite data for arriving at anything worthy of the name of Sanitary Science, or for dealing in an intelligent way with the real causes of our high death-rate.

IV.—*Biogenesis, as bearing upon Decomposition, Putrefaction, and Decay.* By Dr. Robert Bell.

Read before the Society, January 10, 1872.

The object of this communication was to discuss some points bearing upon the decomposition of animal and vegetable substances, with the view of shewing what relationship the development of minute organisms has to this process of decay. Holding it to be demonstrable that putrefaction is relative to the growth and multiplication of, is coincident with, and dependent upon, the development of minute living particles, the author, in the first place, referred to the important purpose served by these minute organisms in the economy of nature, by their liberating the elementary atoms of decaying bodies, and bringing them into a condition for passing again through the same cycle of life, death, and dissolution. If the utility, nay, the absolute necessity, of these microscopic living beings can be proved, this will go a long way towards establishing their mode of origin and development. Certain it is, that the process named putrefaction, decomposition, or decay—and to which may be added disease—is always accompanied, if not produced, by the development of minute living bodies; and the author was thoroughly convinced that decay cannot take effect upon any organism until its vitality is less than that of the germ producing the change, else decay would be found setting in upon living tissues, and disease attacking healthy bodies. The development of these organisms depends also upon favourable external conditions, such as the presence of moisture and of a moderate degree of heat. With reference to disease, is this kind of decomposition not caused by a growth and multiplication of bioplasm in the blood, which is quite foreign to it? The action of the virus of the rattlesnake and the vaccine disease was cited in illustration; and if it were asked in objection to this view, How is it that two persons may be equally exposed to the poison of typhus fever, and only one contract the disease? the answer is, that, in the case of the one who escapes, the vitality of his constitution is unimpaired, whereas, in the other, there is a condition of lowered vitality, incapable of coping with the

disease. In like manner, if it be asked how it is that poisons entering by wounds produce the direst consequences, while those entering by the lungs are frequently innocuous? the answer is, that the part where the poison enters has been weakened by some external agency, and thus prepared to afford nutriment to the invading germs, which therefore multiply at the expense of the weakened tissues, and, as in ordinary local affections, such as a carbuncle or an abscess, the tendency is to depress the entire animal economy. The effect of minute organisms upon living tissues was further illustrated by reference to the fungus which proves fatal to flies, the fungus which attacks the silk-worm, and the blight of the potato. Dr. Bell next directed attention to the causes of decomposition in dead matter, and explained the views of various observers on this subject. In decomposing matter and in putrescible fluids there is developed a series of definite forms; and if a number of vessels containing infusions of different organic substances are exposed to the atmosphere under the same conditions, in all of the vessels forms of life appear, presenting identical characteristics, and belonging to the same families of animalcula. Instead of these forms of life being ascribed to spontaneous generation, is it not more reasonable to suppose that as each vessel is surrounded by the same medium, it is from this medium that the growth of animal forms proceeds—that is, by the deposit of germs from the atmosphere, and their development in the fluid which provides them with a suitable pabulum? This view of the origin and dispersion of these germs had been placed beyond doubt by experiment. The theory of Bastian, that the primordial formation of crystals and living things is essentially similar in kind, was shewn to be untenable. In the case of the crystal, we have the chemical substance of which it is built present in the fluid; and the crystal is deposited according to laws with which we are familiar; moreover, we are able to determine, before the formation of crystals takes place, what manner of crystal will come into existence; or we may prevent its formation altogether by a process which would have no effect on the development of a microzyme—that is, by simply adding water or other fluid to hold the crystal in solution. Another most important distinction (Dr. Bell proceeded to point out) exists in the fact, that cold hastens crystallization, while it retards the development of miscroscopic life. A moderate degree of warmth encourages the growth and development of minute life, and has the opposite effect on the formation of crystals. An excess of heat, however, prevents the production of either. It really savours little of reasoning, not

to mention a knowledge of chemistry, to say that we may attribute the formation of crystals to the presence of invisible germs, just as we trace the development of animalcula to invisible germs. In the one case, we have a definite chemical compound of known composition, which we can obtain manufactured independently of any invisible germs, which we can subject, in some cases, to intense heat without involving its destruction as a chemical substance, or depriving it of its intrinsic properties. We can dissolve and recrystallize it, and we again have it unaltered in any respect. Can such liberties be taken with an animalcule? or indeed with anything endowed with life? Most certainly not. Then, wherein does the analogy consist? They resemble each other only in one very important point, and that is, a crystal of one substance only dissolved in a fluid can be recrystallized into that substance only, and no other; and a fluid containing the germs of Bacteria or Monads only, can give birth to these particular living bodies, and to no other. But Bastian does not stop here. He goes much farther with his hypothesis. He would have us believe that fungus spores also may arise spontaneously. Now, the spores of many fungi can be collected and examined by the microscope. I shew you some, photographed, not larger than 1-3200th of an inch in diameter. These can be planted, and a fungus similar to that on which they grew procured. These bodies are so minute, and of such infinitesimal weight, that there would not be the slightest difficulty in their being suspended in the atmosphere, and carried through the air. And it is beyond all doubt that they are thus transmitted, or I would not have been able to obtain this specimen, which was found on a piece of cheese, for I know it was not placed there by the hand of man. On a square inch of mouldy cheese which I examined, there must have been millions of these minute bodies, and these were all produced in less than a week. Their power of propagation is something wonderful. Why these should be attributed to spontaneous or heterogenous generation, I am quite unable to explain. In organic fluids of different composition we have similar forms of animalcula and bioplasm produced, and these always present the same features, live the same kind of life, and resemble each other most closely in every respect. Yet we are told that their origin is not due to a parent having existed before them. If this particular kind of life is spontaneous or heterogenous, why does it always retain the same features? How is it that we have not constantly cropping up new forms? And how is it that we have not spontaneous origin of larger living beings? Is it because these are so minute, not being visible except

through the microscope, and because we cannot see the primordial germ, that we are going to deny their parentage? I have not the least doubt in my own mind that these tiny beings have a pedigree as remote as any of our aristocracy. *Omni vivum ex vivo.*

As the time at my disposal is limited, I must not further enlarge upon this most interesting part of my subject, but will proceed at once to refer to Professor Huxley's views on it. He evidently has most carefully weighed the evidence on either side, and although he does not insist on the impossibility of living protoplasm having originated at any time from dead matter, yet he is, as far as I can judge, convinced that we have no proof in support of the theory of Xenogenesis; and so far as experiment has taught us, we must in the meantime attribute the origin of these minute living particles, which are the cause of putrefaction, to parents in all respects like themselves. This seems only reasonable, when we ponder the following words, which I quote from Professor Huxley:—

"Prepare a solution (much used by M. Pasteur, and often called 'Pasteur's Solution'), composed of water, with tartrate of ammonia, sugar, and yeast ash dissolved therein. Divide it into three portions in as many flasks; boil all for a quarter of an hour; and while the steam is passing out, stop the neck of one with a large plug of cotton wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay, which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as Bacteria. In a couple of days of ordinary warm weather, the contents of this flask will be milky, from the enormous multiplication of Bacteria. The other flask, open and exposed to the air, will sooner or later become milky with Bacteria, and patches of mould may appear in it; while the fluid in the flask, the neck of which is plugged with cotton wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs, competent to give rise to Bacteria, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould fungi. And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast ash, and water, under no influence but that of free access of air and the ordinary temperatures, re-arrange themselves, and give rise to the protoplasm of Bacterium. But the alternative is to admit that these Bacteria arise from germs in the air; and if they are thus propagated, the burden of proof, that other like forms are generated in a different manner, must rest with the assertor of that proposition.

"To sum the effect of this long chain of evidence:—

"It is demonstrable, that a fluid eminently fit for the development of

the lowest forms of life, but which contains neither germs nor any protine compound, gives rise to living things in great abundance, if it be exposed to ordinary air; while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it, and which may be made visible by appropriate means.

"It is demonstrable, that the great majority of these particles are destructible by heat, and that some of them are germs or living particles capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

"It is demonstrable, that inoculation of the experimental fluid with a drop of liquid, known to contain living particles, gives rise to the same phenomena as exposure to unpurified air.

"And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

"Thus the evidence, direct and indirect, in favour of Biogenesis for all known forms of life, must, I think, be admitted to be of great weight."

The results of the experiments narrated by Dr. Huxley certainly put beyond all doubt the parentage of the enormous quantities of Bacteria developed in the inoculated flask. The same atmosphere was admitted into the several flasks containing portions of a particular fluid, and the vessels were placed in circumstances identical with each other, and yet how different are the results! In spite of this, however, we have scientific men maintaining that these Bacteria are produced spontaneously; that they are so minute as not to require a parent, because we cannot see them actually issuing from the germ. It does not seem to me to be unscientific or unwarrantable to infer from analogy how they originate. The difference in size of the germ or ovulum, which after a certain period becomes developed into the mature child, and then into the adult man, is well known to all. The ovulum measures about the $\frac{1}{120}$ of an inch in diameter, and a man sometimes reaches the height of 6 feet, and may weigh 18 stones. Can our minds compare two bodies of such different proportions? Conceive a man 18 stones—say a Tichborne-weight—to be a microscopic object, and where will the ovule, which was his beginning, be? Why, by some he would be said to be of spontaneous or heterogenous origin. We must not be suprised, then, if we have an animalcule, the smallest specimen of which may only measure $\frac{1}{10000}$ of an inch in diameter, that we

cannot discern the germ from which it sprung. It will not be absurd to suppose that the germs of such animalcula are suspended in the atmosphere by the million. No; to me it is impossible not to feel certain that in every cubic inch of air we have suspended as many germs, which in a few warm days may be the parents of a progeny as countless as the sand. I will mention one other circumstance, known to us all, before I proceed to prove by experiment how decomposition is produced, and whence life in its most minute form emanates.

No one is ignorant of the fact, that cold retards decomposition, and that meat can be preserved for an indefinite period, if frozen and kept at a temperature below the freezing point. We also know that warm weather hastens putrefaction, so that meat which in winter will keep fresh for days, in summer will be quite unfit for food in as many hours almost. Is the rate of decomposition dependent solely upon the higher or lower temperature? Does warmth accelerate decomposition by any intrinsic property which it possesses? The following coincidence will answer the question :—Exactly commensurate with the speed of decomposition is the development of bioplasm. Before decay commences, no bioplasm can be observed on the surface of the meat; and until this bioplasm can be detected, no symptoms of decay manifest themselves. Cold retards the development of microscopic life, as we have remarked before, while it promotes crystallization; moderate warmth, on the other hand, promotes their proliferation and retards crystallization. And it is a coincidence worthy of note that some substances which retard decomposition, in an equal ratio retard the development of minute life, just as cold does, and thus, like cold, by hindering development of the germ; and in some cases, when the agent is strong enough, not only delaying its development, but actually depriving it of vitality. Thus, we have exemplified how disinfecting and deodorizing agents act. Because putrefaction is due to the development of bioplasm, and because the germs of this microscopic life are either killed or have their vitality paralyzed by the action of disinfectants, we have putrefaction delayed, or may be prevented altogether. This fact presents many features of peculiar interest to sanitarians. To demonstrate the influence of a disinfectant upon the development of life, I will relate the results of an experiment upon seeds, and I will give it *verbatim*, as I have it in my note-book :—

On *Friday, March* 17, 1871, I took 12 wide-mouth 2-oz. bottles, and into each placed a little carded cotton half-an-inch in depth. This was saturated with water, and on the top of the cotton in each

bottle a few seeds of common cress were placed. On the orifice of each phial a capsule of tinfoil was fitted, and within the capsule covering seven of them, pieces of lint, saturated with solutions of carbolic acid in water, of the respective strengths, 5 grs., 10 grs., 20 grs., 30 grs., 40 grs., 50 grs., and 60 grs. to the ounce, were placed. The remaining five were covered with capsules containing lint saturated with water only.

Saturday, 18th March.—Applied solutions of carbolic acid to the respective capsules. The seeds shew symptoms of swelling in all the bottles.

20*th.*—Again renewed the different solutions of carbolic acid. No sign of life in any of the seeds.

21*st.*—The seeds in the five bottles that are without carbolic acid are bursting into life. None of those containing the acid vapour shew the least appearance of life, although they are swollen with the moisture. Again, the various solutions were applied to the respective capsules.

22*nd.*—The sprouting of the seeds in the uncharged bottles has made great progress since yesterday, while those containing carbolic vapour are as dormant as ever. No more acid was added to-day, as the lint had lost none of the carbolic odour, which is due to the impervious capsules covering the mouths of the bottles.

23*rd.*—Charged anew each piece of lint. Uniform and rapid growth is going on in all the uncharged bottles, whilst the seeds in the carbolized bottles are quite in the same state as before noticed.

24*th.*—The seeds in the charged bottles are still in *statu quo*, while great progress has been made in the growth of the seeds in the other bottles.

25*th.*—Progress of the cress in the uncharged bottles is steady. It is now half-an-inch high. No appearance of growth in the other phials.

As I think my theory regarding the action of carbolic acid is thus far proved, I did not add any fresh acid, but removed the capsule from the 60-gr. bottle to ascertain whether the dormant state of the seeds is due to actual death or simply to suspended animation.

27*th.*—The cress in the five uncharged bottles is one inch in length, and the roots are ramified through the cotton. None of the seeds in the other bottles shew signs of life; neither is there any vitality manifest in the bottle from which the capsule was removed on the 25th. Removed the capsule from the 5-gr. bottle to ascertain if life still is latent in the seeds.

28*th.*—The growth in the uncharged bottles is still proceeding; but as yet the seeds contained in the bottles from which the capsules have been removed shew no signs of life, and although no more acid has been added to the remaining bottles, they are all in *statu quo.*

31*st.*—Still no sign of life in these bottles, whilst the young plants fill the bottles which were uncharged. All the capsules were removed to-day to ascertain if life still exists in the seeds which have been exposed to the carbolized atmosphere.

April 1*st.*—All the seeds are uncharged, except those in the 5-gr. bottle, which to-day shew faint signs of life, very minute sprouts appearing on some of the seeds.

10*th.*—The sprouts in the 5-gr. bottle have made some, but very little progress, and do not appear as if they would ever develop into cress. None of the seeds exposed to the stronger vapours of carbolic acid have shewn the least appearance of vitality.

12*th.*—Having ascertained the property of carbolic acid, in so far as it checks, and, when strong enough, prevents altogether the maturation of the plant from the seed, I carried the experiment still farther by using in the same way solutions of carbolic acid of 1, 2, 3, 4, and 5 grs. respectively, together with an uncharged bottle.

14*th.*—I find that sprouting has taken place in all the seeds of the uncharged bottles, while in 1, 2, and 3, some of the seeds shew signs of vitality. 4 and 5, as yet, are quite dormant.

15*th.*—It is very interesting to watch the rate of growth in the seeds subjected to the different degrees of strength of the carbolic vapour. Life is apparent in all the seeds, but the rate of growth is altogether in proportion to the quantity of carbolic vapour to which the seeds are exposed. Those contained in the uncharged bottle have made considerable progress, whilst those in the 1-gr. bottle have not made so much; that in the 2-gr. not so much as in the 1-gr., and so on up to the 5-gr., where life is just beginning to manifest itself.

18*th.*—It is curious and beautiful to see the gradation in the growth of the cress. The six bottles contain quite a series of growths; for while the cress in the uncharged bottle is nearly half-an-inch in length, that in the 1-gr. is much shorter; and the rate of growth has been slower in each bottle in exact proportion to the strength of the carbolic fumes which they individually contain, the seeds contained in the 5-gr. bottle only shewing small sprouts.

21*st.*—The rate of the growths is still in proportion to the

strength of the carbolic acid solution, a regular series existing. Another feature in experiment is this,—that some of the seeds in the 4 and 5-gr. bottles are still dormant. Does this imply that there is a delicacy in these seeds? and has the weaker solution of carbolic acid the same effect on these as a stronger solution would have upon all the seeds—strong and weak—exposed to its vapour? I have removed all the capsules to ascertain if the growth will now go on uninterruptedly, and in equal ratio, or whether the growth in any of the seeds has been permanently interfered with, and whether the seeds which at present shew no signs of life will sprout, now that the carbolic vapour is removed from them, and thus demonstrate in what way the acid really acts in a dilute solution; for it has been shewn that it really destroys the germinating power of the seeds when a strong vapour is for any length of time in contact with them.

26*th*. The impression made upon the different lots of seed remains very much the same as when they were in contact with the carbolized atmosphere, as the growth is still going on at about the same rate in the different bottles as it did during the period of their exposure to the carbolic vapour. This is most apparent in the stronger solutions, and naturally mostly so in the 5-gr. bottle, where the growth is quite stunted, and the portion which before shewed no signs of life are only doing so now—and that very slowly indeed. As the strength of the acid diminishes—that is, in the 4, 3, 2, and 1-gr. bottles—the growth is going with greater rapidity in proportion to the weakness of the solution. I have given the results of this experiment in detail, as they are so full of interest and beauty, and point out in a most lucid manner the effect of this disinfectant upon life; and if we may reason by analogy, it indicates to us how it prevents decomposition in different substances, viz., by preventing the growth and development of bioplasm. I may here add that it is in a carbolic acid from the wood which is burnt in the smoking of fish, &c., that the preservative power of the smoke consists. It is volatilized by the heat, and coming in contact with the fish, destroys all germs on their surface, and also by combining with an albuminous substance of the fish, forms a coating of a compound of what I would call "carbolate of albumen," and thus shields the fish from the action of the germs suspended in the atmosphere, and by that means prevents putrefaction, and at the same time gives the fish a flavour.

An experiment in which small pieces of fresh beef were employed yielded similar results as regards the development of life. In this

case the beef occupied the place of the cotton in the experiment with the seeds, and the atmosphere deposited the germs which were acted upon. On August 10th, I introduced small pieces of beef into as many wide-mouthed ℥ii. bottles, into which were fitted stoppers hollowed on the under surface. Into the hollow portion of each stopper I adapted a piece of lint, and saturated one with water, and the rest with solutions of carbolic acid, varying in strength from 1-gr. to 30 grs. to the ounce of water. Having stoppered all the bottles, I set them aside for twenty-four hours. At the expiration of that time no change was apparent in any of the pieces of beef except that which was not exposed to the carbolic acid, and that which was in contact with the 1-gr. solution. In twenty-four hours more, however, all the pieces up to thé 15-gr. solution shewed signs of decomposition; but the remainder were quite odourless till the day after, when the first trace of putrefaction shewed itself. Now, exactly at the rate in which the putrefying odour was developed did microscopic life appear on the respective pieces of beef, so that it was eminently most profuse in the bottle containing no carbolic vapour; and according to the strength of this vapour in the other bottles was bioplasm in more or less abundance. Another noteworthy fact in connection with this experiment may be narrated—that the cells of the bioplasm were much more perfect in form, and more often nucleated, when the carbolic fumes were less strong.

Two weeks after decomposition had commenced, I again examined what *had been* the pieces of beef, for then they were all reduced to pulp, but no life could be detected, the gases emitted from the cells having destroyed their own life. This, however, I can hardly think would have taken place had the decomposing matter been in free communication with the atmosphere, as the foul gases would then have been carried off; but being pent up in the bottles, they seem to have acted as a poison to the bioplasm.

Another experiment illustrates in a most beautiful manner how a disinfectant influences the growth and development of life. A number of similar bottles as those employed in the last experiment were used, and in each a piece of fresh beef was placed. Into the stoppers of two or three of them lint saturated with water was adapted; into the remaining ones lint saturated with various solutions of carbolic acid. Then, on the top of each piece of beef a few freshly laid eggs of the blow fly were deposited, and here, just as in the case of the seeds, life first shewed itself in the bottles containing no carbolic vapour, and the time of the hatching of the remainder was prolonged in exact ratio as the strength of the carbolic acid

increased. In this experiment a discovery which I thought I had made a short time ago seemed to be verified, to illustrate which I will quote almost verbatim from my note-book:—On August 15th, three days after the eggs were laid, those in the bottles containing the vapour from the 40 grs. and 50 grs. of carbolic acid solution have life developed, but there is no appearance of it in the 60 gr. bottle. I have often on previous occasions noticed a great difference in size take place in these larvæ in twenty-four hours, but not so in this instance, as those first hatched are unchanged in size from yesterday morning. Is it the carbolic vapour which stunts their growth? No; because I have observed that freshly hatched ones, when placed on meat which had undergone decomposition for some time, and which consequently was covered with bioplasm, were exposed to carbolic vapour, grew very fast—the carbolic acid, though in a solution of 12 grs. to the ounce, not affecting them in the least for some time; but in a few days they stopped growing and died, and this was due to the disappearance of bioplasm; for when I came to examine what once was a mass of perfect cell life, I found it to contain very few cells indeed, but simply what appeared to be the debris of cells. From this I concluded that these larvæ do not live on decaying matter, as is generally supposed, but subsist on the minute forms of life which bring about the change. This seems to be confirmed by the behaviour of the larvæ in this experiment; for here we have decomposition delayed by the action of the carbolic vapour, and therefore we have the development of bioplasm hindered; as a direct consequence no food is provided for the newly hatched larvæ, which can, no more than anything else, live on air, and so no growth has been observed in them. In the evening I have it noted that life has appeared in all the bottles. This shews that even the vapour of a 60-gr. solution does not prevent germination, although it delays it. The next day I have it noted that the larvæ are not thriving, and in many cases have died, their death being due, I believe, to starvation.

V.—*On the Drainage and Sewerage of Towns.* By Mr. John Phillips, C.E., London.

Read before the Society, February 7, 1872.

The principle of drainage in towns which I advocate, and which was first proposed by me, is called the *separate system.** This was in 1847, when I was chief surveyor of a large portion of the metropolitan sewers. While so engaged, I examined many miles of these sewers with the view to improving their form and construction; experimented on the air-currents in order to determine where and how to ventilate them; and gauged the sewage and rainfall currents, so as to arrive at the sizes of sewers necessary to drain different urban and suburban districts. After many years' labour and study in this employment, I perceived that it was a great mistake to discharge the sewage into the same channels that received the rainfall, as by so doing the ground we lived on, the air we breathed, and the water we drank were being continually polluted; and it appeared to me that the only practical and effectual remedy for this state of things was to discharge the surface and subsoil water by one set of channels into the watercourses and rivers, and to remove the sewage by another set into the country, and there utilize it by irrigation upon, and purify it by filtration through the land.

In my preliminary report, in 1849, on the drainage of the metropolis, I proposed this system for adoption. My plans embraced the conversion of the existing drains and sewers into channels for surface and subsoil drainage, with catch-water sewers to relieve the districts level with and below high-water mark; the diversion of the sewage from the existing surface drains and sewers into separate sewage channels; and the construction of high and low level sewage outfalls, with reservoirs and pumping stations at convenient places along them for filtering, raising, and sending the sewage through pipes into agricultural districts for utilization on the land. But public opinion was not then prepared for this advanced idea; and,

* It is generally thought that Mr. Menzies is the originator of this system. But this is not the fact. I had matured and proposed it nearly eighteen years before he resuscitated it in 1865.

in consequence, my proposal not only met with no support, but with considerable opposition. Time, however, has shewn the necessity for this system, which, I feel assured, will sooner or later be generally adopted. With these few prefatory personal remarks, I will now proceed with my address.

Town drainage is of two kinds—namely, natural or rainfall drainage, and artificial or sewage drainage. The first comprehends the removal of the rain which falls upon the surface, and the water which passes into the subsoil, whether from rainfall or land springs. The second comprehends the removal of human excreta and urine with the slops and waste water from houses; together with the offensive liquids of all kinds which proceed from stables, cowhouses, slaughterhouses, breweries, factories, and other places. By the system of draining towns as now practised both kinds of drainage are removed in the same channels. These consist, first, of drains from the houses and streets discharging into sewers under the streets; and, secondly, of sewers under the streets emptying one into the other until the whole of the rainfall and sewage are received into main sewers, which discharge into the nearest streams or rivers. The progress of this method of drainage has been tentative, the same as that of the steam-engine, or of any other thing or principle that is useful or beneficial to man. As it will be instructive and interesting to trace its history, I will proceed, in the first place, briefly to do so.

Formerly it was the practice generally in towns, as it is now in many places, to throw the slops and liquid refuse produced in the houses out on the surface or into the streets; to retain the fæcal matter in cesspits sunk deep in the subsoil under the privies within or near the houses; and to remove the putrid accumulations occasionally by bucketing them out and carting them away. By this process, which lasted for centuries, the slops and waste water contaminated the surface; the liquid filth oozing from the cesspits saturated the subsoil, and polluted the wells and springs whence water was drawn for drinking and cooking; and the noxious vapours, which exhaled from the surface and from the decomposing filth in the cesspits, poisoned the air breathed by the people. Doubtless this state of things produced or aggravated those terrible diseases and pestilences which periodically ravaged and destroyed thousands of the people, who were thus unconsciously the cause of their own affliction and death.

But in course of time sewers were laid down from the water-

courses or rivers along the streets for carrying off the surface water, as well as the liquid refuse produced in the houses, which, as heretofore, was poured down the gutters or gullies, the houses being still unprovided with drains; and as the districts were extended sewers were continued into them from the old sewers, or from the nearest watercourses; and ultimately the streams or rivulets into which the sewers discharged themselves were converted into main sewers, and arched over. It should be noted that during this time each house had its cesspit for storing the fæcal matter, which was not permitted to enter the sewers; and, as the oldest parts of towns were usually seated high on the sides of ridges or hills rising directly from the streams or rivers, or from the flat grounds near them, the rain falling upon the houses and streets, and the waste water thrown into the streets, descended quickly, and passed away without requiring sewers to remove the rain and refuse water. But, as the base of the slopes and the flat grounds near them were built upon, sewers were carried from the streams or rivers through these grounds and up the slopes, to prevent the decending waters from flooding the lower surfaces. Generally these sewers were laid without much attention being paid to their direction, shape, fall, or capacity; and, in consequence, most of them were inadequate for future extension and the use they were subsequently put to.

This system of drainage prevailed for a very long time, until the springs and wells became insufficient in quantity for the increased populations; and so bad in quality, from being tainted with the percolations from the cesspits, as to be unfit for domestic uses, particularly for drinking and cooking. Then water was brought from purer sources at a distance, and delivered by pipes into barrels or cisterns placed in or about the houses to receive it; sinks were fixed in the kitchens, sculleries, and wash-houses, with drains leading from them into the sewers in the streets; and into these drains the rain water from the roofs, yards, and areas was conducted. Thus were provided street sewers with gullies for removing the rainfall from the surface, and house drains with sinks for removing the waste water from the houses. While this system was in vogue, the drains and sewers, as well as the streams into which they discharged themselves, were comparatively innocuous, because the cesspits containing the fæcal matter were not allowed to be connected with them. It was thought that if this matter was admitted into the drains and sewers, it would deposit therein, and become a worse nuisance than in the cesspits, owing to the small quantity of *waste water* discharged with it being incapable of carrying it away.

But, as water was now supplied by pipes to cisterns in the houses, water-closet contrivances were put over the cesspits, with pipes laid from the cisterns into the pans to keep them clear.* And then, as the subsoil became clogged or unable to absorb the increased quantity of water discharged down the pans, the cesspits overflowed, and were obliged to be connected with the drains communicating with the sewers. Thus was completed the system of combined drainage which is now everywhere in operation, and which is the result, as we have seen, of chance and necessity, and not of deliberate consideration as to the best method of removing and disposing of the rainfall and sewage.

About thirty years ago inquiry was made into the working of this system. It was found that, generally, the drains and sewers were flat-bottomed and excessively large; that the liquids discharged into them were, in consequence, powerless to remove the fæcal matter which continually accumulated, and was periodically removed by lifting it to the surface and carting it away—the occupation of the night-man being thus transferred from the cesspits to the sewers; and that, from the exposed area of the deposit in the sewers, which averaged four times that in the cesspits, noxious gases constantly emanated, and flowed up the drains into the houses and streets, producing fevers and epidemics to a large extent. Then, as a remedy for this evil, various improvements were introduced—such as flushing the sewers instead of cleansing them by hand labour and cartage; putting down egg-shaped sewers with curved junctions in place of flat-bottomed sewers with square junctions; laying house drains and small sewers of impermeable stoneware in lieu of porous brick and stone; and substituting water-closets for cesspits, which were abolished.

That this method of draining and sewering towns, where it has been properly carried out and carefully attended to, is better than any that had preceded it, is proved by the immunity of the populations from those dreadful diseases which formerly attacked them, and by the death-rates as a rule being much lower than they were. So far this is satisfactory. But the question may be asked, does this

* The first patent for a water-closet was taken in 1776. Two years after this Bramah invented and patented the valve-closet, which, with improvements, has been in use ever since. For many years after it was invented only few were used, owing to the expense of the force-pump and elevated cistern which supplied it with water; but, upon the introduction of the steam engine, and of cast-iron pipes, which together enabled the water to be delivered to the several floors of the houses, it came gradually into use.

system perform all the duties imposed on it thoroughly and efficiently? In other words, is the rainfall removed from the surface by the drains and sewers into the streams and rivers as free from contamination as it might be? is the water which percolates into the subsoil carried off from it by the drains and sewers, so as to keep it and the houses free from damp? and is the sewage removed from the houses and other buildings by the drains and sewers without in any way soaking into and contaminating the subsoil, poisoning the air of the houses and streets, and polluting the streams and rivers? These questions involve a consideration in detail of the subjects mentioned, which I will now proceed to discuss.

Surface water, as every one knows, consists of rain which falls sometimes lightly for a few minutes only and barely wets the surface, sometimes moderately for many hours or a day, from a quarter inch to two inches per twenty-four hours, and sometimes heavily for one or two hours at the rate of a quarter inch to two inches per hour. Owing to this variation in depth of rainfall, which causes a nearly equal variation in depth of water running in the sewers, it has been difficult to fix the sizes of sewers necessary to drain town areas, and, in some instances, sewers have been put down too small, but generally considerably too large. This error has been augmented by supposing that a given quantity of rain falling upon a town surface runs off into the sewers nearly as fast as it falls, and that it was requisite to make the sewers large enough to receive and carry off the given quantity of rain in the time of the fall. Now, whatever may be the amount of rain falling upon a town area, the discharge from it into the sewers is never so much nor as fast as the fall upon it. These depend on the duration and degree of the fall, the permeability, friction, and inclination of the surface, and the porosity of the subsoil. From my gaugings in the London sewers, in 1845-46, I found that when rain falls heavily $\frac{3}{7}$ths run off the surface into the sewers as fast as it falls, $\frac{3}{7}$ths continue to run off for many hours after the fall has ceased, and of the remaining $\frac{1}{7}$th, part is retained in hollows in the surface and evaporates, part is worked up with the surface detritus into slush, and the rest is absorbed by the surface soil and the subsoil beneath. From this, and having the areas to be drained, the inclinations of the sewers, and the depth of rainfall per hour, it is easy to calculate the sizes of sewers necessary for draining a town. Thus for draining a street or district of $10\frac{2}{3}$ acres, the inclination being 1 in 250, and the rainfall 1 inch per *hour,* the sewer should be 15 inches diameter at the outlet. With

another district of 14½ acres added to this at a lower level, making together 25⅙ acres, the inclination and rainfall being the same, the sewer at the outlet should be 21 inches diameter. The pipes when running not quite full would drain rather larger areas than when running full. In the former case the rubbing surface would be less in proportion to the body of water than in the latter case; consequently the velocities and discharges would be proportionably increased thereby.

Water in subsoils is derived from rain, which passes into them either vertically or horizontally from the surfaces over them, from higher grounds, or from rivers or streams adjacent to them, and appears as springs, or forms bogs and swamps. Subsoils have a capillary attraction, or power of absorbing and retaining water, in proportion to the degree of fineness and tenacity of the ingredients composing them. For this reason fine tenacious clays absorb and retain water more than friable loams, friable loams more than loose sands, and loose sands more than open gravels; and for the same reason clays when wet are more impervious and cold than loams, loams more than sands, and sands more than gravels. Where, therefore, the subsoils of flat or sloping districts consist of clay or loam, the rain falling on the flat districts lies on the surface or pools, and remains for a long time, until it is evaporated by the sun, or the atmosphere; but the rain falling on the sloping districts runs over the surfaces quickly to all lower levels. Where, on the other hand, the subsoils of flat or sloping districts consist of sand or gravel resting upon beds of clay or loam whose surfaces undulate, are basin-shaped, or lie below the natural drainage levels, the greater part of the rain falling on these districts sinks at once through the surface soils into the subsoils beneath, which become in effect underground lakes, whence it flows over the rims of the impervious beds to lower levels, or into the drainage outlets. Where, again, porous subsoils in low situations derive water from adjacent high grounds, the superimposed beds are usually under pressure from the water beneath, which keeps the upper beds continually wet, and sometimes bursts through them as springs. Again, where impervious beds, which are overlaid by porous subsoils, crop out on the sides of slopes above or level with impervious or porous flat districts, the water percolating down to these surfaces through the porous subsoils, finding free vent at the sides, issue from them as springs or rills, and run over the flat districts, rendering them marshy and swampy; but where the impervious beds with the porous subsoils resting upon them continue under lakes, or the sea,

or through river banks, the water percolating down to them flows freely along them, and discharges into the natural outlets.

The subsoils of towns generally present some or all of the foregoing characteristics. But whether they be impervious or porous, wet or apparently dry, they are so liable to receive water from various sources, and therefore to become and remain wet, that they should always be provided with means for draining and keeping them dry. This is best done by intersecting them with lines of porous drains of brick, or of pipes with open joints: then any water that may be in them, or that may afterwards soak into them from the surface, from adjacent rising grounds, or from streams, will pass into the drains. Clay is impervious to water and air only so long as it is kept saturated. If, therefore, it be intersected with drains the water runs towards and percolates into them, and air follows the water and begins to circulate through the clay, which soon becomes dry, full of cracks, and pervious down to the level of the drains. The effect of under-draining wet clay may be observed by opening a trench in it with a free outlet. In a short time water will be seen trickling from it through the sides, and along the bottom of the trench. The clay at the sides first gives up its water, and then the masses beyond gradually yield up what they contain; and as the water passes out, air enters in by the interstices or crevices that the water occupied, and causes the clay to contract and crack in every direction. Thus by under-drainage the subsoil is freed from moisture down to the drain-levels, air circulates through it, and both it and the surface are made and maintained dry and healthy.

It is almost invariably the practice to erect mansions, villas, and houses without first under-draining the subsoil beneath and around them. This is a great mistake, inasmuch as it always contains or receives water in proportion to its capillarity. When the subsoil is not drained, or is inefficiently drained, the superimposed soil, particularly if it be covered with buildings, continually sucks up water from beneath, which not only makes it wet and cold, and the overlying air misty and chilly, but keeps the foundations and lower walls and floors of the buildings permanently damp, and rots the walls, the wood, and the plastering. Besides this, where the subsoil consists of undrained clay, in summer the moisture freely evaporates, and it dries up, shrinks, and cracks; and in winter the rain enters the cracks, which close up as the clay swells by the absorption of the water. This alternate expansion and contraction of undrained clay causes buildings erected upon it to rise and sink, and the walls to crack and split in all directions—the cracks opening as the clay

swells and the walls rise in winter, and closing as it shrinks and they sink in summer. When excavations are made for buildings, the water in the surrounding subsoil gravitates into the hollows, from being at the lowest levels. In consequence of this the foundations actually always stand upon, or in, damp earth or water. To prevent the damp from rising up the walls, a sheet of lead, a coat of asphalte or of cement, or a course of perforated tiles or of slates bedded in cement, are usually laid upon them just above the foundations. These expedients do not remove the wet from the subsoil, but rather tend to retain it, or to increase it under the floors, through which it rises with greater intensity by the warmth of the rooms. Provision is usually made in specifications for buildings for preventing damp from rising up the walls by one or other of the methods I have mentioned; but directions are seldom or never given for thoroughly under-draining the subsoil, or preventing moisture from rising into it. This is owing, probably, to the general use, of late years, of impermeable pipes in lieu of porous bricks for drains. The former, if securely jointed, can only carry off the surface water and the sewage, while the latter, besides doing this, permits the subsoil water to pass into them, and the sewage to leak out of them. Hence, these systems, when used alone, are inefficient, and prove the necessity of providing one set of channels for removing the surface and subsoil water, and another for removing the sewage.

A complete remedy for the evils arising from wet subsoils is very simple. It consists in putting small permeable pipe drains, with open joints, across and around the sites of the buildings some feet below the levels of the foundations, and in partly filling up the drain-trenches with gravel, broken brick, or stone, well consolidated. Then, any water percolating into or towards the sites would be intercepted by the porous trenches, and pass freely into the drains.* These drains should on no account be connected with the sewage drains, because they would ventilate them, and the sewage gases would be diffused through the air of the buildings. It is extremely desirable, nevertheless, that they should be ventilated. This could easily be done by carrying

* "For thy drayning trench, it must be made so deepe that it goe to the bottome of the cold, spewing, moyst water, . . . which usually lyeth under the cover . . . of the earth in some gravel or sand, or else where stones are mixt with clay, under which thou must goe, . . . if ever thou wilt drayne to purpose. . . . Thou mayest make what improvements almost thou desirest upon thy moyst, cold land if thou observe the directions given."—*The Improver Improved*, by Captain Blith, 1652.

vertical pipes from them up in the external walls of the buildings, with openings into the atmosphere through gratings above the ground. Air would then enter the drains by the gratings on one side of the buildings, sweep through them, pass out by the gratings on the other side, and so keep the subsoil dry and salubrious. If the subsoils of towns were drained and ventilated in this manner they would be considerably less damp, and warmer by several degrees than they now are. By this process, also, clay districts would become nearly as free from damp and as warm as sand, gravel, or chalk districts.

As streams and rivers occupy the lowest parts of natural drainage areas, they are the proper outfalls for the surface and subsoil drainage of towns. Hence from them, at the lowest available levels, main drains should be carried through the subsoil, following as nearly as may be convenient the lines of the ancient watercourses, towards which the surface and subsoil waters naturally gravitate. From these main drains branch drains should be put down through the subsoil on either hand, so as to drain the parts not commanded by the main drains. Then, from both mains and branches, drains should be continued through the subsoil under or near the houses, which drains would form the outlets for the small permeable pipe drains before described to be laid down across and around the subsoil of the houses. Then, into the drains under the streets should be conducted the rain falling on the street surfaces; and into the drains under or near the houses should also be conducted the rain falling on the roofs, areas, yards, and other surfaces belonging to them. Thus would the entire surface and subsoil of towns be thoroughly drained. But in arranging the mains and branches, it is desirable to put catch-water drains along the sides of the higher districts sloping down to the low flat districts, in order to relieve the latter of the upland water which would otherwise flow down upon them and keep them wet; and also to put catch-water drains across the higher part of the valleys and their intervening ridges, so as to prevent the drains in the lower parts from being gorged, and the properties there from being flooded during heavy rains or storms.

An egg-shaped channel is more effectual for the drainage of towns than any other form. The egg-shaped sewer of the proportions now in use in the metropolis and throughout the country, was first arranged and used by myself in 1849.* The main and

* The proportions are the following:—Height, and radius of sides = one and half the width; radius of bottom = one fourth the width. Drawings of it may

branch drains I have been describing should be egg-shaped, but with a much narrower bottom than usual, the radii of the sides being equal to the horizontal diameter, and meeting at a point at the bottom. By this means the more inclined sides would offer greater resistance to the lateral pressure of the earth, and the contracted channels would concentrate and heap up the rills of water, increase their velocities and scouring power, and so prevent sand from the subsoil and detritus from the streets from depositing. The channels, up to the levels of the ordinary runs of water, should be formed of solid fire-clay blocks in long lengths, accurately moulded, highly vitrified and glazed; and the sides above the blocks, as well as the crowns, should be constructed of good hard bricks, laid in cement or hydraulic mortar. The drains from the houses should consist of glazed stoneware or fire-clay pipes with butt-joints, the lower halves of which should be bedded in cement in half-collars, and the upper halves should be left open for the subsoil water to enter them. The sizes of these drains should be 4 inches, 5 inches, or 6 inches, according to the areas to be drained. In order that the influx from the branch-drains may not deflect and obstruct the flow in the recipients, all the main and branch junctions, and all the house drain junctions, should be curved or oblique pointing down the streams. The fall of the branch-drains should be as much as possible at the higher parts. From thence it may diminish into the mains, as well as along the mains, as the quantity of water increases and the streams unite.

As regards the sizes of the main and branch drains, they may be calculated as follows:—Calling the horizontal diameter D, the area A, the wetted perimeter P, the hydraulic mean depth H, the fall in feet per mile F, the velocity in feet per minute V, the quantity of water discharged per cubic feet per minute Q, and the number of acres of covered surface the conduit will drain N; then $A = D^2$, $P = D \times 3.66$, $H = \frac{A}{P}$, $V = 55 \sqrt{H \times 2 F}$, $Q = V \times A$, and $N = \frac{Q}{26}$. The value of the last expression may be gradually altered to $N = \frac{Q}{20}$ (according to the experience and judgment of the engineer), as the conduits increase from two feet diameter, and the inclinations advance from twenty feet per mile. The conduits would discharge the water

be seen in the "Suggestions Relative to Main Sewerage and Drainage," issued by the "Local Government Act Office," at the Metropolitan Board of Works office, and in Hurst's and Molesworth's Handbooks.

from a fall of rain of one inch in depth per hour. It should be noted, taking a single conduit with drains entering it at intervals along the sides instead of at the head, that the water by pouring into the stream *along the sides*, as it usually does, accelerates the velocity and discharge of the stream, and as a consequence causes it to occupy less capacity in the conduit than it would do if the whole quantity entered *at the head*. Attention to this circumstance enables much less sizes of drains to be used for conveying away the rain from covered town areas than are usually put down.

The one merit of the old method of draining towns by porous brick drains and sewers was, that while they carried off the rain from the surface they thoroughly drained the subsoil, and rendered the sites dry and healthy. The brick drains laid under the houses from back to front kept the surfaces free from water and the subsoils free from damp; and the brick sewers laid under the streets did the same there. While therefore they received only this water and the waste-water from the houses, which together were comparatively innocuous, they performed their duty most efficiently, as neither absolutely foul water nor foul air escaped from them. But directly excremental drainage was admitted into them, the subsoil became excrement-sodden, the air of the houses excrement-tainted, and the streams and rivers excrement-reeking; and they have been a difficulty and a trouble in this respect ever since, and will continue to be so as long as this irrational system—polluting the entire network of drainage channels—is suffered to exist.

It has been persistently insisted on by the General Board of Health that perfect drainage of towns would result from using impermeable pipes with sealed joints. This is a great fallacy. For, as *drainage* consists in removing the surface and subsoil water, and *sewerage* in removing the sewage, the General Board of Health, by jumbling the two together, have accomplished neither perfect drainage nor perfect sewerage; and it is utterly impossible for them ever to do so while combining rainfall and sewage in the same channels. If sewage only were received into impermeable pipes with sealed joints, the pipes being always of sizes proportional to the space occupied by the sewage, they would quickly transmit the sewage from the houses to the outfalls without depositing, leaking, and emitting but a small amount of noxious gases, on the way. For accomplishing this object, and this only, these pipes are most admirably adapted. But if they be laid in a wet subsoil, or even in a dry one, with perfectly sealed joints, they cannot under any circumstances drain the former, nor the latter when water perco-

lates into it, because the water can neither pass through the material nor the joints. Hence the modern system of draining and sewering towns solely by impermeable pipes, with water and air-tight joints, fails to drain the subsoil—yet this, as I have before shewn, is one of the essential objects of town drainage. And it is a fact that in those towns where only such pipes have been put down with sealed joints, the subsoil is waterlogged and cold, the overlying air is moist and chilly, and the foundations and lower floors of the houses are damp and rotting. It has been sought to make the pipes perform the duty of removing the subsoil water as well as the sewage, by leaving the joints wholly or partly open for the subsoil water to pass into them. But by this expedient rills of sewage pass bodily outwards through the same openings, and run either under or along the sides of the pipes, saturating the subsoil beneath, and bubbling up, when it is full, through the joints at lower levels.

In the "Suggestions Relative to Main Sewerage and Drainage," issued by the "Local Government Act Office," we are told that "house drains should in all cases be laid in a water-tight trench," that "if the subsoil is porous the trench should be lined with clay-puddle," and that "the pipes must be securely jointed." The object of these "suggestions" appears to be to prevent the sewage from passing through the joints and saturating the subsoil, which therefore must be left without drainage. With all due deference to the authority who made these "suggestions," it is quite as necessary to provide for draining the subsoil of towns, so as to keep it as free from damp as possible, as it is to provide for removing the surface water and the sewage, which these pipes, when properly laid and securely jointed, only can do. But with regard to draining the subsoil, we are told by the same authority that "sewers and drains in wet subsoil should be made to act as land-drains." Now, against this practice, which was that of the old Metropolitan Sewers Commissions, the General Board of Health very properly objected, because through the same channels which allow the land water to pass into such sewers and drains, the sewage passes out of them, and contaminates the subsoil and the wells.

Hence, by the impermeable pipes as now put down for sewers and drains, two evils occur: where the pipes are securely jointed the subsoil remains wet, and where the joints are left wholly or partly open the subsoil water is let into them, but the sewage is let out of them. Thus the evils which beset the old system of porous brick sewers and drains—by the liquid sewage percolating through them outwards,

and the solid portion depositing inwards—beset the new system as well, or rather worse of the two. These evils arose then as they arise now—from discharging the sewage, which is a daily invariable quantity, and the surface and subsoil water, which is always a variable quantity, together in one set of channels instead of in two sets—namely, one for removing the sewage, undiluted by rainfall and excessive factory liquids, into the country for utilization and purification by the process of irrigation upon, and filtration through the land, and the other for removing the surface and subsoil water, with the purified factory liquids, into the streams and rivers. Until this is done there never will be perfect sanitary drainage and sewerage of towns.

As a remedy for the evils arising from a wet subsoil, when impermeable pipes with water and air-tight joints are laid in it, the General Board of Health, in 1852, recommended that it should be drained by small permeable land drains, as practised in draining fields, with outlets into the impermeable sewers and drains which received the rainfall and sewage; and in many instances this combination of permeable pipes with open joints for subsoil drainage with impermeable pipes with sealed joints for sewage drainage has been carried out. That it is highly desirable, and even necessary, to deep drain the subsoil of towns by a system of small drains with open joints, as I have described, there can be no doubt; but that these drains should be connected with the drains and sewers which carry off the sewage, there is, in a sanitary point of view, a fatal objection. The tendency of under-draining the subsoil, as I have before observed, particularly if it be clay or loam, is to effect its disintegration, by causing innumerable fissures and cracks to diverge through it from the drain trenches, down to the levels at which the drains are laid, and so rendering it permeable for the passage of water and air through it—for where water will pass from the surface down through the subsoil into the drains below, air or gases will pass from the drains up through the subsoil into the atmosphere above. If, therefore, the subsoil drains be connected with the sewage drains, as was recommended by the General Board of Health, the noxious vapours emanating from the sewage would, by reason of their lightness, pass through the joints of the subsoil pipes, rise through the fissures and interstices of the earth above, and escape into the houses, the warmth of which would materially accelerate the draught. Thus would the air of the houses be always imperceptibly but surely contaminated by the sewage gases, which the inhabitants would be continually inhaling; and such

has been found to be the case where this system has been adopted, and doubtless fevers have been produced or intensified in consequence.

In regard, therefore, to the subsoil of towns, on sanitary and economical grounds, it is highly important that it should be thoroughly drained—not only for maintaining it in as dry a state as possible for health, but for preserving the houses from damp. It is also highly important, in order to prevent the air of the houses from being polluted by the sewage gases, that the subsoil drains should not be connected with the sewage drains, but that they should discharge into the drains which receive and convey the surface water into the natural streams and rivers. The air circulating in these drains, and escaping from them through the gullies, sinks, and other openings in or near the surface, or even through the subsoil under the houses, would be practically innocuous, because they would receive no water containing matter liable to ferment and emit deleterious gases.

The next question for consideration is that referring to the condition of the rain water as it passes from the house and street surfaces into the sewers, and thence into the rivers. I have described how, in my opinion, this water should be collected and removed; but it is important for the sweetness of the sewers and the rivers that it should be as free from contamination as possible. This depends on the care and attention paid to the cleansing of the streets; for it is obvious that the cleaner they are kept the cleaner will be the water running off them. Now, in most towns and parts of towns, the state in which the streets are permitted to remain in regard to cleansing is positively disgraceful! They are let alone from one to six days and more at a time, when, if they are dry, the dirt is swept up, and if they are wet, the slop is scraped or swept into heaps, and carted away. Hence, in windy weather, clouds of dust, consisting of triturated granite and horse droppings, are blown into the houses and into the eyes and mouths of the wayfarers; in moist weather the surfaces are covered with a greasy paste of the same composition, which renders walking on the footways a kind of retrograde progression; and in rainy weather the carriage ways are rivers of slush, which passing vehicles splash over the footways, and from head to foot over persons walking on them.

It was declared by the General Board of Health that as the droppings of horses on highways, the deposit of soot and the droppings of birds on roofs, and the droppings of animals on yards, contained valuable manure, it was justifiable to convey these matters away with the house drainage through the common sewers. It was

partly owing to this declaration that the pernicious practice of removing the rainfall and sewage in the same channels was maintained, and that the streams and rivers were continued as direct outfalls for the sewers, and converted into the elongated cesspools that many of them are. Horse droppings, when compounded with stone detritus and over-diluted with rain, are of small value to the farmer as manure; and with regard to the soilage of water from roofs by soot and bird droppings, this is so insignificant that it need not be taken into account. What, therefore, remains for consideration is the best method of removing the droppings of animals from, and the cleansing of the street surfaces.

It was proposed by the General Board of Health to deluge the street surfaces daily with water by the hose and jet, so as to wash the horse droppings and the street detritus down the gully shoots into the sewers. Apart from the questionable sanitary advantage of saturating these surfaces so often as daily, and apart from the questionable value of the manure when diluted largely with water and sandy matter, it should be considered what effect this process would have on the sewer channels. It is observable when rain falls heavily or continuously for some hours, on paved or macadam highways, especially highways of large traffic, that the water as it runs off the surfaces carries sand, silt, mud, dung, and other heavy material with it down the gullies into the sewers. Now, owing to the weight and clammy nature of these matters, they deposit and cake along the sewer-bottoms forming wide flat surfaces, which spread the sewage, render it shallow, increase its friction, impede its flow, and diminish its power to remove the fæcal matter, which also deposits in consequence. This is invariably the condition of all the branch sewers, where the currents are small and weak; but in the main sewers, where the currents are generally large and strong, the bottoms are swept clean. If, therefore, the street refuse deposits in the sewers, owing to the inefficient street cleansing as now practised, cleansing them by the hose and jet every day, every other day, or twice a week, and permitting the refuse to be washed into them, would largely increase the deposit, and require manual labour to remove it.

To give a succinct idea of what, in my opinion, would form a perfect system of cleansing the street surfaces of towns, I would state that it is desirable to have a double set of catch-pits, or settling tanks, into which the rainfall and the hose and jet washings should be passed before the water was permitted to flow into the streams and rivers. First, I would place at all the street gullies small catch-

pits into which the rainfall and hose and jet washings should be received and subside prior to the water overflowing into the sewers;* and secondly, to make assurance doubly sure, I would construct large settling tanks at or near the outlets of the main sewers, for the water to pass through them, before permitting it to discharge into the natural water-courses and rivers. By this double process of arresting the surface water it might be confidently hoped that the supernatant water would at last contain nothing that would prejudicially affect the streams and rivers—in fact, that it would be as comparatively free from noxious impurities as that proceeding from the surface of ordinary rural districts.

But, in order to render this plan still more perfect, it would be advisable to organize a system of sanitary police for cleansing the streets of towns, whose duty it would be to sweep all the streets daily, and remove the horse droppings as they fell; to periodically wash and water the streets, both carriage-ways and foot-ways, particularly courts, alleys, and closes, with the hose and jet; and, after every rainfall and street washing, to empty the catch-pits at the gullies and the settling tanks at the sewer outlets of the detritus. If this system were adopted, the money at present expended in intermittently sweeping up and carting away the slop, and in watering with carts to lay the dust, might be more efficiently employed in constantly sweeping the streets, and occasionally washing and watering them with the hose and jet. As this process would prevent the formation of mud in wet weather, and of dust in dry weather, it would be a great boon to the inhabitants, especially to shopkeepers; and the large quantity of dung and detritus now passing into the sewers and rivers would be kept out of them. The value of the street sweepings as an agricultural manure would, to a large extent, cover the cost of its collection.

In most of the cities of China and Japan large numbers of men, women, and children are employed daily in the roads and streets, and along the banks of the canals and rivers, with baskets and small shovels, picking up horse-droppings and other matters suitable for manure, which they sell to farmers. In this way they obtain a livelihood, and keep the public ways clear of organic refuse. We

* In 1848 I invented a trapped gully catch-pit for preventing the street detritus from passing into the sewers, and the sewer gases from escaping into the streets. Thousands of these were then, and have ever since been put down n the metropolis and other towns. Drawings of it may be seen at the Metropolitan Board of Works office; and also in the "Minutes of Information on Drainage and Sewerage," issued by the General Board of Health in 1852.

might very advantageously adopt this system in our towns. The refuse, as collected, could be brought to convenient depôts and paid for at once, arrangements being made with farmers to fetch it away for spreading it on their lands. By this plan hundreds of poor persons would earn a competent subsistence, and the streets, sewers, and rivers kept free from pollution.

Having described how best, in my opinion, to drain the surface and subsoil and dispose of the water, and how best to cleanse the street surfaces and dispose of the refuse, the next subject for consideration is, how best to remove and dispose of the sewage.

The present practice is, as I have already stated, to discharge the sewage into the same drains that receive and carry the rainfall into the water-courses and rivers. It results from this practice that although the sewage now passes from the houses along the drains and sewers into the streams and rivers more speedily and completely than it did formerly, yet on its way large quantities still leak through the open joints of the pipes, as well as through the porous brickwork into the subsoil; that, owing to the enormous sizes of the conduits as compared with the small quantity of sewage ever running in them, noxious gases are generated in the space unoccupied by the sewage, and the same escape, not only by the inlets, but by the open joints of the pipes through the ground into the houses and streets; and that the sewage, when it arrives at the outlets, is poured into the streams and rivers, where it continually ferments and gives off deleterious gases, and the mechanically suspended matters deposit and form putrescible mud banks. Hence, as stated in the opening of this paper, the ground we live on, the air we breathe, and the water we drink, are all perniciously affected by the system of drainage now in vogue.

But I may be told that the saturation of the subsoil by the sewage, and the contamination of the air by the sewage gases, escaping through the open joints of the pipes, would not take place if they were made water and air tight when the pipes were laid. Now, it is found in practice that, owing to there being a joint round each pipe at every short distance of two feet, to the difficulty of getting under so many joints to securely stop them, to the ground under the pipes sinking from the sewage leaking through the joints, and to the cracking of the jointing—1st, by filling in and ramming the ground round the pipes; 2nd, by the ground under the socket-bearings sinking from the weight upon the pipes being concentrated at these points; 3rd, by the vibration of the ground in which the

pipes are laid from the passage of heavy vehicles over it; and, 4th, by the contraction and expansion of the pipes from their varying temperature, they are seldom made, or if made, are never maintained perfectly water and air tight. In many cases, too, the pipes are jointed with clay which, from not adhering to the glaze, becomes soddened with the sewage, and is washed or forced out of the joints; and very frequently only the upper halves of the joints are stopped, the lower halves being left open. Moreover, owing to the short lengths of the pipes, they are often laid in hollows, or undulating, instead of to regular inclinations; and, owing to the large space in, and the irregular form of the sockets, and to the diameters of the butt-ends being much less than those of the sockets, the cement or clay used in stopping the joints is pushed into the pipes, producing protrusions and ridges, around which solid matters accumulate, and the liquid sewage is heaped up and escapes through the cracks and openings. Hence through the joints, from one or all of these causes (some of which are only avoidable by extraordinary care, and some are not), both sewage and sewage gases constantly escape; so that, while the sewage sinks into and saturates the ground beneath, the gases emanating from it rise through the permeable ground above, and issue through the chinks of the flooring and skirting, and the linings of the soil-pipes into the houses, the heat of which promotes the draught. Having had as much experience in sewerage as most men, I am sorry to say that this is what really takes place, more or less, from every line of pipes as now put down.

We are often annoyed by drain-smell in houses, and puzzled to know whence it comes; and sometimes children and adults are attacked with typhoid fever, scarlet fever, diphtheria, or diarrhœa, in consequence. When search is made for the place whence the smell proceeds, it is generally found escaping from cracks or openings in the joints of the drain-pipes, from imperfect connections of the soil-pipes with the drains, or from perforations in the soil-pipes themselves. Now, as these pipes are usually made of lead, in a short time they become perforated by the corrosive action of the gases emanating from the decomposing excreta which collects in or adheres to them. The perforations are small at first, but they gradually grow larger, until the pipes become quite honeycombed and rotten with them. At night, and in cold weather, when windows and doors are closed, and gas and fires are burning, the heat of the houses draws the poisonous gases through the perforations into the rooms at a fearful rate, causing fevers, and perhaps death to some of the inmates.

Hence it is a question whether architects and builders should not at once cease to use lead soil-pipes, and use glazed stoneware pipes instead. If cast-iron were used for this purpose, probably it would oxidize (unless it were galvanized) and become full of holes quicker and worse than lead. Now soil-pipes are the vertical continuation of the horizontal drains; and, as stoneware has proved to be the best material for the latter, so would it be for the former, as the excreta would not adhere to it, nor the gases corrode it. Soil-pipes need not be more than four inches diameter, even for stacks of closets; and if the material were three-fourths of an inch in thickness, the pipes could be made six feet in length; and proper curves, junctions, and traps could also be formed for connecting the closet pans with them. The joints should be slightly conical, ground to fit accurately, like stoppers in bottles, and luted with a thin cement, to prevent the gases from escaping. These pipes should also be carried above the roofs of the houses, with openings into the atmosphere. They would then become main channels for ventilating the sewage pipes from the sinks and closets.

In regard to the noxious gases emanating from fresh sewage flowing in "good tubular self-cleansing drains and sewers," it has been asserted by the General Board of Health that, as fæcal matter does not begin to decompose for two or three days after it is produced, and that, as it is removed at once in such channels, deleterious gases do not generate in nor escape from them, and, therefore, that neither trapping the inlets nor ventilating the channels is necessary. This statement, that fresh and flowing sewage does not emit foul vapours, is a great fallacy. Now, it is a well known fact that, when animal and vegetable substances are exposed to heat, moisture, and air, they decompose and evolve noxious volatile gases much more rapidly than they otherwise would do. Of fæcal matter rather more than one-fourth is solid, and nearly three-fourths are liquid. Hence, soon after it is discharged into the sewers, it is readily dissolved by, and its constituents enter at once into chemical combination with the liquid sewage. Then, owing, 1st, to the heat of the sewage; 2nd, to the heat evolved by the chemical combination; 3rd, to the exposure of the sewage to the large air space above it which is provided for the rainfall; and, 4th, to the agitation of it which is caused by the flow, fermentation is rapidly accelerated and increased, and noxious vapours are generated from it into the overlying air before the fæcal matter, dissolved in the sewage, leaves the sewers. Then, as these vapours are much lighter than common air, and extremely subtle, they immediately rise up

the drains to the water-traps under the kitchen, scullery, and wash-house sinks, and the water-closet pans, through which traps they escape, not only every time water is discharged down them, but when the pressure of the pent-up gases inside is greater than the air-pressure outside, and permeate the air of the houses.

Every sewer, therefore, as now put down, whether it be self-cleansing or of deposit, is an elongated retort, the sewage is the organic compound from which the foul gases are generated, and the drains which convey the sewage into it from the openings in the sinks and closet-pans are the necks from it conveying the gases up to the same openings, through which they pass into the houses as just described. Hence it follows that it is as necessary to provide proper ventilating pipes from the upper ends of all sewage pipes in order to carry off the poisonous gases as they ascend, as it is to provide the sewage pipes themselves to take off the sewage. Unfortunately this most essential detail in house drainage has been almost neglected; and there is indubitable evidence that the whole train of zymotic diseases, if they have not been actually caused, they have been enormously intensified in consequence. As, however, the evil is now well understood, it is to be hoped that in future sanitary and building Acts of Parliament ventilation of the drains and sewers, and separation of the sewage from the rainfall, will be strictly enforced.

It should be borne in mind that sewage gases are generally warmer and lighter than common air, that this warmth and lightness are increased by the warm water which is discharged down the drains, and that consequently the gases naturally ascend to and strive to escape at the highest points of the sewage drains, which are the openings into them from the sinks and closet-pans. In regard therefore to the ventilation of these drains, two essential objects are to be kept in view,—namely, one to prevent the gases from escaping through the inlets to them into the houses, and the other to convey the gases from them into the external air over the houses. The first object is to be attained by placing proper water-traps in the sewage pipes directly under the openings to them, and the second by carrying ventilating pipes from the upper extremities of the sewage pipes, over the outlets of the water-traps, through the adjacent walls to the house-tops, or into the closet soil-pipes, which should be continued above the houses—such pipes to open into the atmosphere there, away from the chimney-tops and attic windows. The effect of this arrangement would be that, as the gases emanating from the sewage ascend the sewage pipes, they

would rise at once up the ventilating pipes into the stratum of air passing over the houses, and not accumulate as they do now at the water-traps, and ultimately pass into the houses. Then, as all the drains from the houses and all the sewers have an air communication with each other, and as the sewage and gases in the drains and sewers are of varying temperatures, and the ventilating pipes rising from them would be of varying altitudes, the action of the various temperatures and pressures upon each other would be such that in some of the ventilating pipes the currents would be downward, and in the remainder they would be upward; and thus would a complete circulation of air through all the drainage channels be established without the possibility of the gases escaping into the houses through the water-traps. But it is an indispensable condition that both sewage and ventilating pipes should be everywhere perfectly air and water tight, otherwise, if there be cracks, fissures, perforations, or openings in them at any points, foul gases will most assuredly escape at those points, especially during the night and in cold weather, when gas and fires are burning, and doors and windows are closed.

Sewage streams by agitation and contact with air tend to purify themselves. Many years ago my attention was called to this by the state of some open sewers running through fields from the villages of Kensall Green and Streatham. When the streams left the villages they were black and smelt badly; but as they progressed through the fields they gradually became almost clear and inodorous. This was caused, not by the putrescible matter depositing along the channels, but by the agitation by the flow, and the exposure to the air, liberating and dissipating the noxious constituents. The same thing goes on more or less in every drain and sewer in a town, the amount of deleterious gases escaping from the flowing sewage being in proportion to the air surface in contact with it, or air space over it. From this it is obvious that the less the sewage is exposed to air, and the less air space there is above it,—in fact, the nearer the sewer capacity is brought to the sewage capacity—the less gases emanate from the sewage. This desideratum is to be attained by altogether excluding the rainfall from the sewage channels, and then reducing their capacity to the capacity or maximum flow of the sewage. But by making the sewers, as we do, large enough, and often many times larger than are requisite, to accommodate the rainfall, the sewage, which occupies only a small space at the bottom of the sewers, is necessarily spread out and exposed to a large body of air, which promotes its decomposition

and the emanation of gases from it, whether it be flowing or stagnant; and then, owing to the extreme volatility of these gases, they flow up the drains and bear on all the traps through which they escape, as before described. Thus the ramification of sewers and drains, as now put down, is neither more nor less than a vast gas-producing and gas-distributing apparatus, and in the air thus continually polluted we are doomed to live. The remedy for this evil consists, as already stated, in excluding the rainfall from the sewage, and then reducing the sizes of the pipes to the maximum quantity of sewage, the evaporating surface of which, and the amount of gases emanating from it, would then be reduced to a minimum. Such small sewers would never choke up, provided they were constructed with perfectly cylindrical pipes of stoneware, in long lengths, and with improved conical joints securely sealed. The sewage currents themselves could be made to ventilate these sewers, by causing them to descend on revolving fans, with pipes carried from the fans above the altitude of the houses. As the gases emanate from the sewage they would be drawn into and driven up the pipes by the fans.

All experience proves that the liquid sewage which proceeds from houses in towns has sufficient body and power to remove the excreta with itself immediately, completely, and imperceptibly, when both are discharged into pipes of proper form, size, inclination, and construction, and innocuously when the pipes are properly ventilated; and that by no other method can the excreta be so quickly, inoffensively, and cheaply removed. But the great mistake that has been made—that has produced all the evils we suffer from foul earth, foul air, and foul water, and has brought the system of removing the excreta by water-carriage into disrepute—is discharging the sewage into the rainfall and subsoil water drains. To counteract the trouble and difficulty we experience by this practice, a host of ingenious contrivances, consisting of traps for penning back the foul air; ventilators with absorbents for filtering it; closets with dry earth, charcoal, and other substances for deodorizing the excreta; and chemical mixtures with filters for cleansing the sewage, have from time to time been adopted. But these expedients are merely local palliatives. None of them go to the root of the evil and provide a complete and effectual remedy. This would be accomplished, in my opinion, by removing the sewage by itself in a system of branch and main pipes, which should be as separate and distinct from the drains which remove the rainfall and subsoil

water as the water-supply pipes are from the gas pipes. This separate system of pipes, and the disposal of the sewage, may be briefly described as follows:—

Where the water supply pipes terminate in houses—namely, at kitchen, scullery, wash-house, and other sinks, and at closet-pans—the sewage is discharged. Hence, from the openings in these receptacles pipes should be carried as at present into main pipes, which should be laid along the back or front of the houses. At all the inlets water-traps should be placed as at present; and from the extremities of the sewage-pipes, over the outlets of the water-traps, air-pipes should lead into main ventilating-pipes, which should be carried above the house-tops. The pipes from the sinks should be two inches diameter, and those from the water-closets four inches diameter. This size, from ordinary houses, should be continued into the main pipes outside the houses, with proper curves at all junctions and changes of direction. The sizes of the main pipes should be regulated according to the fall and the maximum quantity of sewage to be received by them, the minimum size being five or six inches. The main pipes should communicate with each other until they deliver the sewage into one or more outfall pipes. These should be continued into the country, where the clarified liquid sewage should be utilized, after extracting the solid and mechanically suspended matters from it, in growing crops by irrigation, and purified by filtration through the land, the surface of which should be prepared and the subsoil underdrained for its reception. And, finally, the effluent water issuing from the underdrains should be discharged into the nearest streams or rivers, as it would then be perfectly pure.

I have already described how the main pipes may be ventilated by the sewage currents driving revolving-fans with air-pipes carried from them above the houses; and how the liquid sewage, if it were discharged into properly formed pipes, would remove the excreta with itself without deposit. If, however, it were thought desirable to flush the pipes occasionally, this could be easily and cheaply done by continuing pipes from the water-supply-pipes over the sinks, and at the closets, into the sewage pipes below the sinks and closet traps, with stop-cocks placed in the connecting-pipes under the bib-cocks, so that by turning the former at any time the pressure of water in the supply pipes would be brought to bear in the drains. By this simple expedient every branch and main drain in a town could be thoroughly flushed. It might be adopted with great advantage in connection with the present system of drains. The traps under *sinks* in areas and yards of houses, usually become unsealed in

dry, hot weather, and noxious gases escape from them. If such sinks, or the drains from them, were to be trapped at their outlets in the main drains, where water is almost constantly flowing, the traps would be always sealed, and effluvia would not escape through the sinks.

The amount of sewage flowing in the sewers of a town is never the same through the day and night. During the morning, as people become active, and a general washing-up and emptying of slops take place, the flow gradually increases and rises to a maximum; but in the afternoon it gradually subsides, and during the night it nearly ceases and falls to a minimum. From gaugings which I made in the metropolian sewers in dry weather, in 1845-46, I found that of the whole quantity of sewage due to the twenty-four hours about one-half was discharged in six hours—from 8 A.M. to 2 P.M.—and nearly one-fourth in two hours in the middle of the day. This may be called the *maximum sewage discharge.* From this, and having the quantities of sewage discharging from particular streets or districts, as well as the inclinations of the pipes, the sizes of pipes necessary to remove the sewage may easily be determined.

The consumption of water in towns varies according to the habits and quality of the populations, and to the trades and manufactures carried on therein. In towns with a pipe-water supply the mean quantity used by the working classes for drinking, cooking, and washing of all kinds, hardly amounts to eight gallons per head per diem; while the average consumption by the middle and higher classes does not exceed twelve gallons per head. But an additional quantity, amounting probably to about five gallons per head, is used in ordinary trades and manufactures. Hence the general *domestic* consumption of water in towns may be taken at fifteen gallons per head of the populations. This water, by the various uses to which it is applied, is converted into genuine sewage; and it is remarkable that the organic matter which it contains in solution requires this amount of dilution to render the sewage suitable for growing crops. But there are towns from which double and treble this quantity of water is turned into the sewers. This over-dilution renders the sewage much too weak, indeed almost worthless, for manurial purposes, and doubles and trebles the cost for works, for removal, for land, for labour, and for management. The excess comes chiefly from factory and other works, and much is due from careless use of the water, from leaky pipes and taps, and from bad closet fittings. How, therefore, to deal successfully with the enormous quantity of sewage produced in these towns is a question difficult to solve. There

appear to be two modes of doing this—one to collect the sewage into large reservoirs, therein to extract its impurities by a chemical process, and then to discharge the clarified liquid into the streams or rivers (which process at best is not to be relied on); and the other, to collect it into a separate system of drains, to supervise the water-pipes, taps, and closet fittings, so as to reduce the leakage, waste, and use of water to a minimum, and to exclude the rainfall as well as the greater part of the factory liquids from it. As much of the latter is not actually sewage, and is capable of being purified in tanks on the factory premises, this should be done, when it may be discharged with the rainfall into the surface water drains communicating with the streams and rivers. By this means the sewage would never be over-diluted, and the quantity would be reduced within a manageable limit for removal to, and application on the land.

Nothing is more harrassing to the inhabitants of towns than to be continually annoyed by defective drainage and sewerage, and to be endeavouring periodically to abate fevers and epidemics occasioned thereby. It may be said that "it is not now possible to revert to the separate system in towns, owing to the surface being drained, and the sewage from the houses being carried off by the single or combined system." Nothing therefore remains but to continue the present abominable system as to the removal of the cloacal refuse. When, however, it is determined to effectually remedy the existing state of things, no system of works would lead to that end so surely and so satisfactorily as that of the double system of drains, which could easily be arranged and constructed, and the sewage utilized and purified by the process of irrigation and land filtration, as I have proposed. And with reference to the present drains and sewers in towns, they could be made to conform to this system without much difficulty and expense. It should be borne in mind that, as regards the sewerage and the disposal of the sewage, a great evil is to be remedied, that palliatives act only locally and temporarily, and are wasteful of means; and that it is far better, and in the end much cheaper, to work in accordance with sound principles that will lead to permanency and success. Clear and defined plans with these objects in view could easily be laid down, and the works carried out as circumstances and funds permit.

No people prize the value of liquid manure for its fertilizing properties so much as the Chinese and the Japanese. With them to save and to use it for raising all kinds of crops, except rice, in order to feed their teeming populations, is of the highest importance. For this purpose they keep large earthen tubs in or about their houses,

and put all their excreta and urine into them. By rich and poor this is looked upon with the utmost complacency. In the canals and rivers, on whose banks their towns are placed, they moor long boats, into which the manure stored in the tubs in the houses is emptied daily by men who carry it in buckets from the houses to the boats, the housekeepers selling it for money or vegetables. The boats when full are conveyed to agricultural districts, and emptied into water-tight tanks sunk in the fields, the manure being covered with straw to prevent the ammonia from escaping. Besides removing it in this way, trains of men and strings of horses, who bring vegetables to the houses every morning from gardens outside the towns, return with it, each man carrying two buckets, and each horse two conical tubs of it. Also at convenient situations in the streets and roads there are latrines, with earthen tubs under them, from which the manure is similarly removed daily. As may be imagined, the stench at times is most sickening—that is, to us fastidious Europeans; but nothing would astonish a Chinese or a Japanese more than hearing any one complain of stench either from the earthen tubs in the houses, the buckets in the streets, or the boats on the canals or rivers. Before using the manure they dilute it in the tanks with from ten to fifteen times its quantity of water. When thus prepared, they carry it in buckets to the growing plants, round whose roots they pour or scatter it with a ladle. The crops so raised are noted for their quick growth, large size, and rich quality.

This system of collecting the excreta and urine has its advantages, in so far as the manure is not wasted and lost, but is economized and made profitable; the drains do not receive it, and therefore do not emit noxious gases into the houses and streets, and the subsoil and streams are preserved from pollution. But it also has its disadvantages, in so far as the sight and smell of the filth in the tubs, and the daily removal of it in the buckets, are concerned; and in the offensive gases which escape from the tubs and permeate the dwellings. With care, however, this system would answer well in villages and suburban districts, as also in villas and detached dwellings having gardens or fields of sufficient extent upon which to use the sewage. Thus, if farmers or cottagers, instead of throwing it away, or allowing it to run to waste into drains and streams, were to collect, dilute, and apply it in cultivating the crops, it would not only stimulate their growth and largely increase their production, but prevent the streams, as also the air and subsoil of the dwellings, from being contamimated. The receptacles for the manure might

consist of water-tight tanks, placed below or above ground, as far as possible from the residences, with hoppers or moveable covers for pouring in the manure, close-fitting floats for keeping air from it, pipes for drawing it off when properly diluted for distributing it on the land, and ventilating pipes for carrying the effluvium rising from it high into the air overhead.

In towns, however, it would not be safe to adopt this system; for the storage of the manure within or near to the houses, together with its removal from them, would be so nauseous to sight and to smell as to be almost unbearable. It would be bad enough in high-class houses, but in low-class it would be far worse, because storing it within rooms or places provided for its reception would be sure to lead to more dirty habits than now obtains. Moreover, the receptacles would be neglected and overflow, and the gaseous emanations from them would soon produce epidemics similar to those which formerly depopulated towns and villages. Hence, to return to any such system as this in towns, even in the best form that could be devised, would be to retrogress instead of to progress in sanitary improvement. Bad, however, as the Chinese and Japanese system of keeping earthen tubs in their houses and emptying them daily may be, it is preferable to our old system of keeping large leaky cess-pools in the ground near our houses, and emptying them once or twice a year; because by the former process the matter is removed fresh before it begins to putrify and emit noxious gases, whilst by the latter it was not removed until it had decomposed and given off deleterious gases, which the people continually inhaled.

The *dry-earth system* is the Chinese and Japanese system improved. Under the closet seat a moveable pan is placed, into which dry-earth falls from a box at the back, and covers the excreta and urine each time the closet is used. By this method the dry-earth absorbs the ammonia and gases, and no smell escapes, provided the pan is frequently emptied. This plan is suitable in places having no pipe-water supply, as also in cottages and farms, and in country or suburban mansions or villas having large gardens where sufficient fresh earth could be obtained, and the contents of the pans used as manure. But it is impracticable for houses in towns, from the difficulty, expense, and inconvenience in procuring and storing the necessary quantity of earth, and also from the attention requisite to supply the boxes and empty the pans. This attention might be given in large dwellings where servants are kept; but the people generally in houses, especially houses occupied by two or three families, with only one closet between them, neither could nor

would give it the attention it requires, and in consequence it would become a nuisance.

There is also what is called the *filter-system*. This consists of a filter of cinders and ashes resting on a perforated screen placed above the bottom of an iron tank. The excreta, urine, and water discharged from the closets pass into the space at the bottom of the tank, under the filtering medium. Here the excreta remains, while the urine and water filter upwards and flow off at top into the drains—the excreta, cinders, and ashes being removed occasionally in carts. This plan is suitable for factories, barracks, workhouses, large detached buildings, and blocks of houses, where a supply of water is at command, and the tanks are accessible for regulating and emptying them. But it is impracticable for houses in towns, owing to the inconvenience of having the filters in or about the houses, and the expense and attention necessary to regulate them and periodically cart away the contents.

These and cognate systems are useful for special purposes and places. But as, after all, they deal with the excreta and part of the urine only, there must be in addition a system of drains and sewers to remove the surface and subsoil water, the house and chamber slops, and the liquid refuse from factories. Hence such systems involve divided and costly appliances, supervision, and labour, without adequate advantages. Moreover, they partially resume the old objectionable cesspool system; for the excreta is retained where it is produced for various periods (a process of deodorization or filtration being applied between production and removal, to prevent it from becoming a nuisance), and it is ultimately removed by hand labour and cartage. Besides, the house slops and waste water, and the factory liquids, would still prejudicially affect the subsoil, the atmosphere, and the rivers. It should be borne in mind that as sewage, which contains every kind of organic matter, is being continually produced, and soon after production begins to decompose and emit noxious gases, it should be removed immediately. And, of all methods of removal, none is so expeditious, so convenient, and so healthful as that of discharging it through pipes. Where this system of removal has been properly carried out, it works so efficiently, so satisfactorily, and tends so much to cleanliness and refinement, that it could not well be superseded. It is true it has great defects; but as these arise from improper arrangements and construction, by improvements in these respects it would work as perfectly as other schemes of human invention.

Of late years, the question of how best to dispose of the sewage

has been a most debateable one. Hitherto, as every one knows, the sewage has been poured into the streams and rivers, which have become so filthy in consequence as to be almost unbearable; and, as they are daily becoming worse with the increase of populations and the growth of manufactories, some other method of disposing of it must soon be adopted. Various processes of treating it—some mechanical, some chemical, and some by irrigation—have been in operation for several years past. It would be instructive, and tend to a solution of the question, if these processes were strictly inquired into and explained. But I can now only state the nature and results, as time is advancing, and I fear I have already wearied your patience.

The mechanical methods consist in arresting the solid and flocculent matters contained in the sewage by subsidence, strainers, and filters, in a series of tanks or reservoirs, and then allowing the liquid to pass into the watercourses and rivers. These methods have all turned out complete failures—the effluent water still containing the original amount of organic putrescible matter in solution, and consequently polluting the streams and rivers into which they discharge nearly as bad as before.

The chemical methods aim at separating the polluting matter from the liquid by the application of various chemical preparations, and then manufacturing the residuum into a marketable manure. These methods, with one or two exceptions, have also proved unsuccessful—the effluent water being still charged with a large proportion of soluble putrescible matter, which renders it unfit to enter the rivers and streams; and the manure, besides costing by the manufacture nearly as much as it is worth, being almost wholly deficient of ammonia, which constitutes its value for agricultural purposes. With respect to the exceptional methods referred to, although they do not completely purify the sewage, yet they clarify it sufficiently to warrant their adoption in connection with towns situated on tidal rivers, estuaries, or the sea, inasmuch as the greater part of the organic matters and the ammonia are retained and manufactured into a profitable manure; and what is soluble in the effluent liquid is immediately dissipated and becomes harmless by the excessive dilution. So far, then, both mechanical and chemical means have failed to completely extract the noxious matters from the liquid; and, as there is no hope that chemistry will ever effectually purify it, we are driven to seek for a solution of the difficulty in the process of irrigation. In this, from the experience gained, there is great hope of ultimately arriving at success.

Irrigation was largely used, as a means of fertility, by all ancient nations. It is still extensively practised with most beneficial results in China, Japan, India, Italy, France, and Spain. We have been slow to recognize its merits, because we have been ignorant of its true principles and effects, and have never until lately properly used it. Where we have adopted it, it has failed in some instances to give satisfaction. This was because we did not know that sewage irrigation requires to be conducted differently to water irrigation—namely, to first extract, by precipitation, the solid matter from the sewage, and then to pass the clarified liquid *through* the land, and not run the *raw* sewage *over* it—that, in fact, downward filtration through the soil must accompany irrigation upon it.* We have selected stiff clay land without under-draining it. By so doing it became water-logged and chilled; the fertilizing elements in the sewage, instead of being absorbed and retained by the soil for feeding the roots of the crops growing upon it, were dissipated into the air and lost; the crude sewage deposited and decomposed on the surface, and gave off offensive vapours; and the effluent liquid ran off the surface into the water-courses with great part of the putrescible matter in solution. We have selected open or porous sandy and gravelly land, some of which we have under-drained, and some not. On the former we have used the sewage, after extracting the solid matters from it, by the ridge and furrow system, and on the latter by the catch-water system. By these processes we have had increased production of crops; but the effluent water, whether issuing from the under-drains or out of the subsoil, or running off the surface into the adjoining ditches, still contained an undue amount of polluting matter, which the porous and siliceous nature of the soils failed to absorb and retain for the crops to assimilate. We have more recently selected mild clayey or loamy land, the surface of which we have laid out in ridges and furrows with gentle slopes, deeply pulverized the top soil so as to form a thick porous bed between the surface and the subsoil for receiving the sewage and cultivating the crops, and deeply under-drained the subsoil so as to convert it into an effectual natural filter. By this treatment the soil, from the surface down to the level

* The solid matter, or slush, extracted from the sewage, may be manufactured into a portable manure, or, more profitably, into Portland cement by General Scott's process; the sulphate of alumina and milk of lime, which are added to the flowing sewage to precipitate the mechanically suspended matter, when the sewage is subsequently brought to rest in settling-tanks, being suitable for either production.

of the drains, has become permeable and aërated; the alumina in the clay or loam has absorbed and retained from the clarified descending sewage its ammonia and other fertilizing properties, which the roots of the growing crops have greedily appropriated; and the effluent water, after passing through the filtering body, has issued from it and the under-drains clear, bright, and inodorous—as clear and pure, in fact, as crystal water bubbling from the rock. The success that has attended this method of irrigation combined with filtration leaves no doubt that herein is to be found a solution of the difficulty of disposing of the sewage.* And if we will only set about excluding from the sewage proper, first, the rainfall; second, the factory waters; and third, the water which is wasted by leaky pipes and taps, and bad closet-fittings, we shall then reduce, first, the quantity of sewage to a manageable compass; and second, the dilution of the organic matter to a degree of fluidity suitable for the crops and the land together to utilize and purify it. At the present time, owing to our habits, interests, and ignorance in arranging, constructing, and using drains and sewers, the sewage is a drug, a nuisance, and a difficulty. But the time will come, as populations and their food wants increase, when the sewage will be everywhere carefully collected into a system of pipes of its own, and used for fertilizing the land, as I proposed should be done in my report on the drainage of the metropolis in 1849. The annual produce of farms will then be at least double what it is now; and the wonder will be how we could have gone on cultivating crops without availing ourselves of this grand source of benefit for the country at large.

Before concluding, I am desirous of saying a few words with the view to improving the tubular pipes as now made for house drains and sewers. Superior clay pipes, truly cylindrical, highly

* "Rich land-water is ever the washing . . . from great towns, houses, or dung-hills, the riches whereof is invaluable. . . . Thou mayst continue this water . . . working upon thy land almost all the yeare round, provided thou keepe it not too long upon a place, for thou must have an especiall eye that thou soake not thy land too much, for then . . . it will . . . exceedingly prejudice thy hopes. I speake not to advise thee to continue thy water . . . long upon one place, but be ever removing it from place to place. . . . Keepe thy land rather in a thirsting condition, not glutted ready to spewe it up again. So mayst thou preserve thy land greene and fruitful, . . . and a weekes grasse, or a load of hay, be worth three or foure. I myself, by these opportunities, have cut twenty-foure load where I cut but five or six the yeare before. The directions exactly followed, I will lose my credit if thou faile of the effect promised."—*The Improver Improved*, by Captain Blith, 1652.

vitrified, and suitable for the conveyance of water or sewage, were first manufactured in this country in 1821, at the Southwick Pottery, near Sunderland.* The largest pipes then made were five inches diameter, but the length was four feet, the thickness one and half inch, and the pipes sustained a hydraulic pressure from 291 lbs. to 350 lbs. per square inch before bursting. The clay pipes now made are only half this length, half the thickness, very irregular in shape—no two pipes being ever precisely alike—and imperfectly burned. When they are put together to form a drain or sewer, the irregularity of shape and the multiplicity of joints produce a succession of undulations and protuberances which check the sewage current, facilitate the emanation of gases from it, and cause hair, paper, rags, and other things to catch in the joints, fæcal matter to deposit, and sometimes the drain or sewer to stop up. Besides this, owing to the immense number of joints, many of them crack, break, or are left open, as before mentioned, and through the fissures the sewage and noxious gases constantly escape. The thickness and length as now made were fixed originally by the manufacturers, because, as they said, the pipes *could not be made thicker and longer.* It is owing to this that town drainage has not been carried out so well as it might have been; and it is astonishing that superintending inspectors, engineers of towns, and local boards of health, should have remained so long apathetic, and not before this have demanded, or themselves introduced, a greatly improved article.

In regard to the manufacture of these pipes, if more attention were paid to the chemical composition and mechanical preparation of the clays; if the thickness were made one-half more than the present thickness, or about one-seventh of the diameter; and if, while drying, the pipes were pressed by a machine a second time, so as to make the substance as dense and the bore as perfectly cylindrical as possible, they could be made in four, five, and six feet lengths, instead of two feet, as at present. There should also be a conical raised ring outside the butt-end and inside the socket-end of each pipe—the former to be turned and the latter bored, either before or after burning, as found to be most practicable. The pipes would then fit into each other as perfect as ground stoppers in decanters. The pipes should

* Stoneware pipes were first made for house drains in the metropolis in 1846, by Mr. William Northen, of the Union Pottery, Vauxhall Walk, Lambeth; and the first to use such pipes in the metropolis, in place of bricks for gully and house drains, was myself. This was in 1846.

be salt glazed while burning, and the whole thickness completely vitrified. These essential details manufacturers should attend to, in order to produce really good pipes fit for sewage drainage. Such pipes, when laid in the line of a drain or sewer, would form a perfect and continuous cylinder, without the slighest wave or protuberance inside, and would be superior to cast iron pipes. The conical joints would fit accurate, and the open parts of the joints should be securely stopped all round with a specially prepared cement, to prevent leakage. With these pipes the drains and sewers of the separate system, as also of that now in vogue, should be constructed. It is true, they would cost more than the present pipes; but I contend that the efficiency of the drainage channels should not be sacrificed, as it now is to a large extent, to the short length, irregular shape, and slovenly manufacture of the pipes as now made. Pipes in four, five, and six feet lengths, truly cylindrical, and with close-fitting joints, such as I have described, could be made. It rests, therefore, with engineers, architects, and surveyors, to say that they shall be made, and that they will suffer none other to be used in their works. The result of using them would be, quick and complete removal of the sewage without leakage, and improvement in the health of towns.

In conclusion, I would beg to offer a résumé of the deductions I have arrived at in this paper:—

First. That it is expedient that the subsoil of towns should be thoroughly drained, so as to render the houses erected thereon perfectly free from damp.

Second. That it is expedient that the surface water should be collected into channels, with which the subsoil drains should be connected, but from which the sewage drainage should be carefully excluded, so that the water conveyed thereby should be discharged into the natural water-courses or rivers in a practically innocuous condition.

Third. That the sewage drainage should be removed in separate channels or pipes of dimensions not larger than necessary for its passage, and should be conveyed to positions outside the towns by gravitation or pumping, and there clarified by chemicals in tanks, and afterwards distributed by irrigation over land made fit for its reception by deep surface pulverization and deep subsoil drainage, or similarly clarified before permitting it to enter a stream or river.

Fourth. That a system of Sanitary Police be established, whose duty it should be constantly to collect the horse droppings and surface refuse, which should be removed and deposited upon the

land; and further, to render the street surfaces perfectly clean by continually sweeping them and occasionally washing them.

Fifth. That, with the view of reducing the volume of sewage for irrigation and filtration within manageable limits, it is expedient that the water supply fittings, and the water supply to closets, should be supervised, in order to prevent leakage and waste, and needless use of water; and that the liquid refuse in factories should be purified in tanks on the factory premises, and then discharged with the rain and subsoil water into the streams and rivers.

Sixth. That, with the view of more readily adopting the system of sewerage which I have advocated, it is expedient that sewage farms be obtained, upon which the sewage should be discharged by irrigation after passing through the necessary preliminary stage of extracting the solid matters from it.

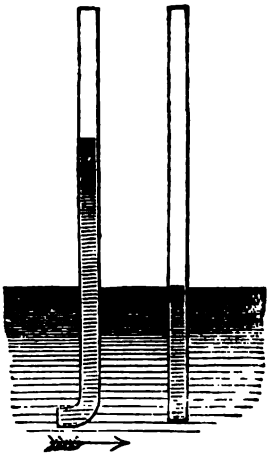
Seventh. That, with the view of effectually preventing the escape of noxious gases into the houses, it should be incumbent upon the owners or occupiers to provide air-pipes from the sewage pipes, carried above the level of the highest part of the houses, away from chimney tops and attic windows.

If these principles were adopted, I venture respectfully to submit, that as perfect a system of town drainage and sewerage would be provided as in the present condition of sanitary science we should be able to secure.

VI.—*On Pressure Logs for Measuring the Speed of Ships.* By James R. Napier, F.R.S.

Read before the Society, 15th March, 1872.

During the Session of 1854, I described to this Society an instrument which I had designed in 1847 or 1848 for measuring the speed of ships. It was an application to ships of the principle which Pitot, a celebrated French Mathematician, had applied in the year 1732 to the measurement of the velocity of rivers, &c. Pitot's instrument consisted of two open-ended glass tubes, the one being straight, and the lower extremity of the other bent to a right angle. These two tubes, fixed to a frame, having been placed vertically with their lower extremities in the current, in such a manner that the open end of the one faced the current, and of the other was parallel to it, were expected to give its velocity, by applying the height to which the water would rise in one of the tubes, in the well-known formula $V^2 = 2gh$. This arrangement of Pitot's was unsuitable as a log for ships at sea, for although the difference in the heights of the water in the two tubes might remain constant for the same velocity, their positions relatively to a fixed scale would vary with every immersion and vertical motion of the ship, and so their heights in the tubes could not be ascertained with sufficient accuracy. To remove these objections I connected the upper ends of Pitot's tubes with the ends of a U or bulb tube containing mercury. And to increase the scale, as well as to prevent any but the forward motion of the ship affecting the height to which the mercury would



now rise, Pitot's straight tube was bent back, the openings in the two tubes now looking in opposite directions. This was the instrument I designed in 1847 or 1848. I was puzzled then how to get quit of air, for I desired to have the instrument on deck where I conceived it would be most useful, and had no idea of using compressed air in the manner which was afterwards patented by Mr. Berthon. A suggestion of Mr. Robert Mansel's in 1850 led to the construction of the instrument referred to in the paper of 1854. But the difficulties experienced in keeping the air out, arising either from bad workmanship or from other causes, made me abandon the idea of fixing the instrument above the water-level, and place it well below. In this position it worked beautifully and without trouble, on all the vessels to which it was then applied.

Mr. Mansel's suggestion was to dispense with the U tube of mercury, but use the two connected water tubes and partially pump the air out—so much air as to raise both water columns high enough for observation. An instrument on this plan was tried about the year 1850, on the S.S. "Laurieston," belonging to Messrs. Jardine, Mathieson & Co. The results were unsatisfactory, and I preferred using the mercury and sucking the *whole* air out. This arrangement of Mr. Mansel's, however, has since been extensively and delicately used in France by Darcy and by Bazin for measuring the velocity of water in canals, &c.

It was to be expected that by connecting Pitot's two tubes as I had done, with their open ends parallel and looking in opposite directions, and with water alone in the connecting tubes between the sea and the mercury, that the pressure on the mercury in the two branches of the U tube would be equal for every immersion and motion of the ship except that which it was desired to measure, namely, the forward motion, and the height to which this pressure would raise the mercury would shew the speed, and such, I believe, is the case, for the speed is now shewn by this instrument with a degree of accuracy hitherto unknown.

Mr. Berthon, as already stated, appears to have been working upon the same idea, and to have succeeded in overcoming the objections to Pitot's tubes for marine uses, by connecting them, as I had done, with a U tube of mercury, but using such a volume of air (as the medium between the mercury and the sea) as would produce the range of scale he desired, for in December 1849 he patented this arrangement. His previous patent a few months earlier, however, shewed that he had then no idea of using two tubes.

I have made the foregoing statement before shewing some modifications of my original instrument,—1*st*, Because it was here I first described what I then called a speed indicator for ships; 2*nd*, Because a claim has been set up by Mr. Berthon, and by others for him, as the originator of the Pressure Log, excluding my claim to at least an independent if not prior invention; for although I am indifferent as to the priority, I am not so as to the independence and originality of my own; and 3*rd*, Because the Pressure Log has been recently patented by a Mr. Fletcher, and a paper communicated to the British Association at Edinburgh, describing it as Fletcher's Rhysimeter, the concluding part of the paper stating "that hydraulic pressure tubes for measuring the speed of ships have been adopted by Pitot, Darcy, Berthon, and Napier, but hitherto they have not been extensively used in sea-going vessels." It is not for me to say what Pitot did with his instrument one hundred and forty years ago, as the only account I have seen of it is his paper of 1732, in the Transactions of the Academy of Sciences, wherein no allusion is made to ships, and Darcy, describing his own modifications in 1858, asks, "How it is that this invention of Pitot's had not been applied by engineers? How it came to be considered a pure speculation which had not been practically useful?" &c., and in none of his papers alludes to any one having applied the principle to ships; and 4*th*, Because Mr. Fletcher, although he had seen me three months or more before reading his paper, and had seen all my papers on the subject, and one of my instruments, left it to me to tell the General Committee of the British Association that the instrument for which he and others wished to get £30 of the funds of the Association for the purpose of experimenting upon it, was mine and not Mr. Fletcher's—was about twenty years old, and had been described eighteen years ago to this Society. That the Pressure Log has not been extensively used in sea-going steamers may be true, but the insinuation that this was due to defects in my arrangement of it at least—which virtually the same instrument under another name, the "Rhysimeter," obviates—is not to be admitted.

I am indebted, however, so far to Mr. Fletcher's action in this matter for making me reconsider my old instrument, and which has resulted in certain modifications which I shall now shew.

In my original instruments I had a cock at the highest point of each of the Pitot tubes, for the purpose of either sucking the air out if placed above the water level, above the height due to the velocity, or for allowing it to escape of itself if placed sufficiently far below;

but finding so much difficulty in keeping them free from air, and having other engagements, I placed them latterly below the water-level, and left them to take care of themselves; for by opening both cocks the air, wherever it came from—bad joints, bad workmanship, or otherwise—easily escaped.

I recently found, however, that by a peculiar arrangement of a single cock, the instrument might be with equal efficiency placed as far above the water-level as formerly it had to be below it; for then it had to be as far below the surface as the height due to the velocity, so as to prevent air entering by one of the cocks when it was opened, and this position was frequently inconveniently low. By the new arrangement (fig. 3), when the water rises by the motion of the ship in the one tube (Pitot's bent one), it is allowed to escape through the axis of the cock into another tube to the bilge, and in its passage and descent it sucks first the air, if any, then the water up the second tube (Pitot's straight one), so that the cock may be opened or shut at pleasure, and the instrument by this simple means surely freed from air.

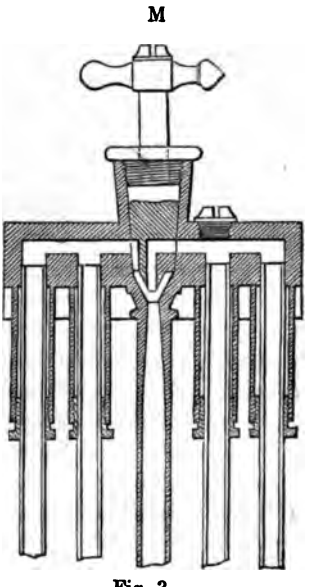
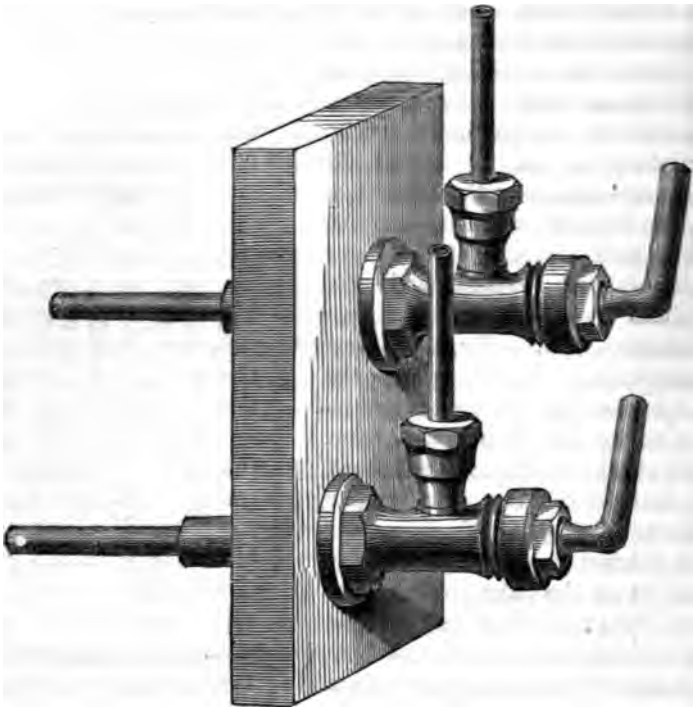


Fig. 3.

The diagrams (figs. 1 and 2) give a general idea of the Log as recently made and applied to a ship. A and F are nozzles placed near each other about the bilge, or well under water, and in iron vessels are screwed into the plating against a copper washer; they can be removed at sea and replaced by those who have courage, as the hole into which they are screwed is not $\frac{3}{4}$-inch diameter. They project about three inches from the ship's side, with a small orifice in each, and correspond with the ends of Pitot's tubes. The plane of the orifice in F looks forward, that in A formerly looked aft, but recently has been parallel to the vessel's course. The side tubes above *c* and *d* are of glass, in order that any air may be seen, for it is essential to accuracy that there be none between the sea and the mercury. After this has been removed by opening the

cock M for a few seconds, and the cock again shut, the instrument is at work and shews the speed by the height to which the mercury rises. When it is placed about the water-level, the waste-pipe, *f*, is led into the bilge. When it is to be used on deck, *f* has to be



The Nozzles.

attached to the condenser of the engine, so that the air may be sucked out without trouble, by merely opening the cock M. The scale can easily be formed from any observation of the height of the mercury at a time when the rate of the ship is known, the formula $V^2 = mh$ giving the height of mercury for any speed; but it is more conveniently made by means of a diagram, such as fig. 4, in which the velocities marked on the horizontal line O, X, are the square roots of the distances from O according to any scale. The observed height of the mercury at any speed is set up from O, X, at its proper distance from O, and a straight line drawn through

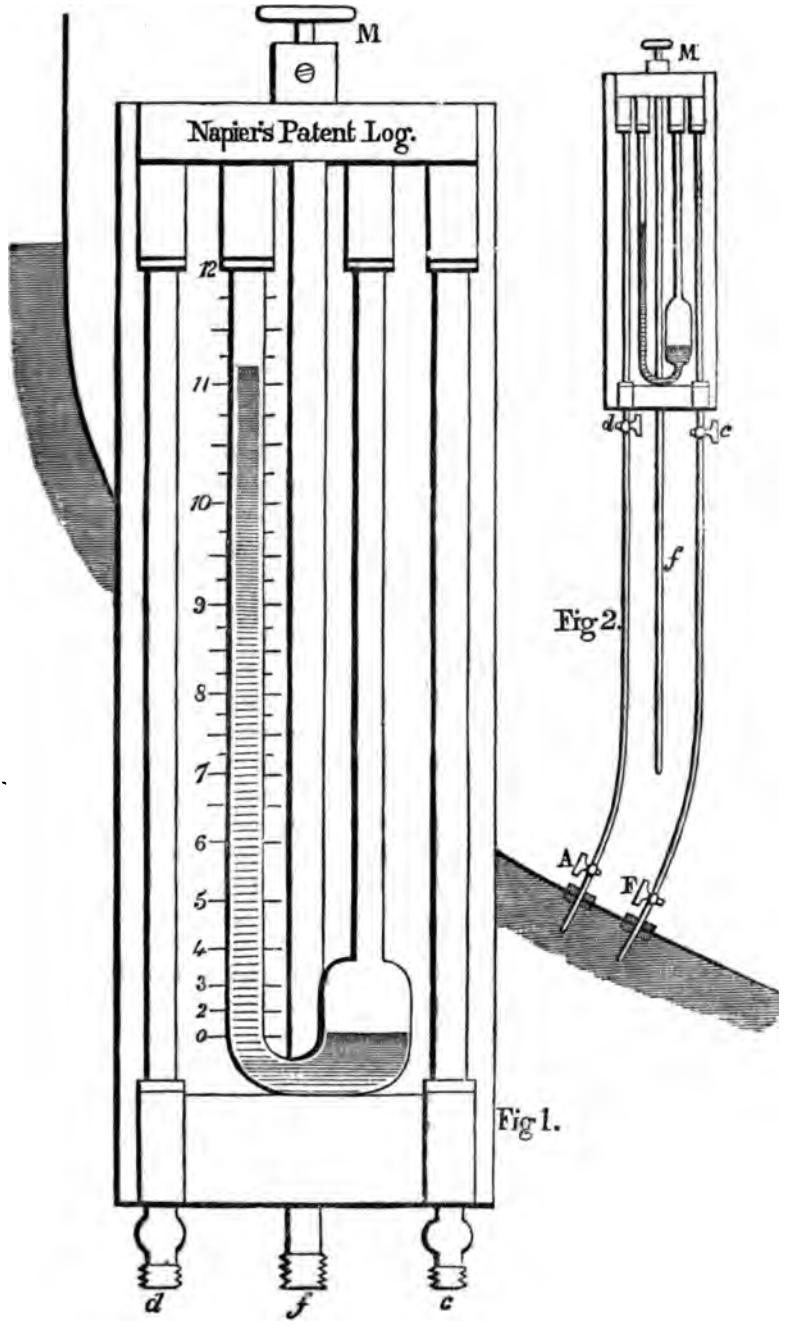


Fig 1.

Fig 2.

O and this point shews what the height of the mercury would be or any other speed. Or if it be assumed that the nozzles are in

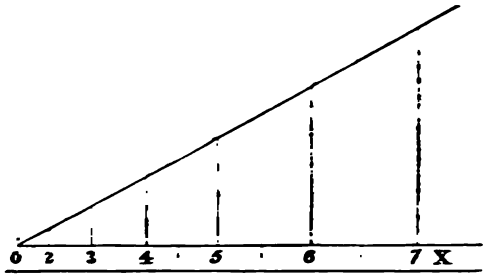
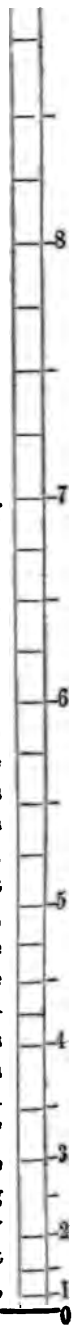


Fig. 4.

still water relatively to the surrounding water, and so constructed that the fall in the one tube equals the rise in the other, then the scale can be constructed or calculated from the formula $V^2 = 11.45h$, where V is the velocity in knots per hour, and h the height of mercury in inches. The height for a given speed may vary in different ships with the form and position of the nozzles, but when these are fixed in any position in a given ship, the scale is constant. The instrument is equally accurate at low as at high speeds, only at low speeds the intervals of the scale are less. Part of the steam yacht "Imogine's" scale is shewn full size on the margin.

In all the instruments I had made previous to last year, I had not succeeded in getting a scale longer than that due to a single bent tube with its orifice looking forward, and sometimes it was less; whereas I expected, whether reasonably or not, with the addition of Pitot's straight tube bent back, or in the opposite direction to his bent one, to have got double the range. But, while trying the instrument with Sir William Thomson in his yacht "Lalla Rookh" last year, one of the sailors remarked that the mercury rose higher when the sea forced the tubes out of their fore and aft direction into one more across the ship. The explanation was easy, the orifice looking aft was in an eddy, caused either by its own tube or its companion, it was—in relatively still water—in water which was going with the ship, at the ship's speed. The instrument subsequently put into the Earl of Breadalbane's yacht "Imogine," proved that this explanation was correct; for there the after-tube was not bent back but left straight—the plane of the orifice being parallel to the vessel's course—and the scale realized was nearly double of what a single bent-tube with its orifice facing the current would have been. Mr. Berthon, when experimenting with his log,



made some curious observations on the effect which the position of the orifice had on the mercury column. When its plane was at right angles to the vessel's motion and facing the same direction, the mercury rose 4 inches; when the plane was parallel to the vessel's motion, the mercury fell 6 inches; and when it was at right angles to the direction of motion and looking back, the mercury fell 2 inches; when at $41\frac{1}{2}^{\circ}$ to the vessel's course, it was 0. Sir William Thomson and Dr. Joule made similar observations by blowing air on a sphere —finding the greatest depression when the orifice was at the equator with its plane parallel to the direction of the current.

Darcy, with the convenient and simple arrangement he had for measuring the velocity of water in different parts of the same cross-section of pipe, made many experiments, with the plane of the orifice in the three directions, and found similar results. Bazin in his experiments on the flow of water in canals, found the depression with the plane of the orifice parallel to the current to be equal to the rise when the orifice faced the current. Whoever first discovered the fact is unimportant now. Some youngsters racing across a pond in a leaky tub discovered that when the bung-hole or leaky side was parallel with their course the tub leaked least, and one of them, years afterwards, invented that beautifully simple apparatus, the Ejector Condenser. Some marine engineers have observed that a cock in the engine-room, and well under water, which ran freely when the vessel was at rest, gave no water with the vessel at full speed. Such Mr. Wm. Muir informed me happened in the first "Iona," and I suspect it is the case with many engineers that they cannot get sufficient condensation water with the vessels at speed, with cocks and pipes abundantly large, and are ignorant of the cause. Professor James Thomson, of Queen's College, Belfast, made the principle more intelligible to engineers when he described his jet pump, and shewed that the action was produced by fluid friction. The cock M referred to above is merely another application of the same principle. Chemists might take advantage of it by using the same cock in iron for the construction of a mercurial exhaust-pump, more simple and perfect than Sprengel's. Architects might remember the principle in their designs for chimney-tops, and save the disfigurement of many fine houses.

In passing, I might ask a question which I observe has been asked by others,—viz., What is it which a pressure gauge, inserted in a pipe in which a liquid is flowing, shews when the condition of the orifice is not known?

I have yet to get experience of the effect, in a heavy sea, which

the orifice parallel to the ship's course has upon the oscillation of the mercury, whether this is a better position of it than the original one; this oscillation is in any case greatly reduced by making the orifice in one or both the tubes small enough relatively to the size of the tube in which the mercury rises.

The distance to which the tubes ought to project from the ship's side, so as to be in still water relatively to the ship, is not known, nor is it essential they should project so far, as the only effect which the distance is known or supposed to have, is on the magnitude of the scale. At, or on, the ship's side, the water may be going with the speed of the ship, when there would be no rise of the mercury; when projected a sufficient distance, the rise would probably be that of twice the height due to the velocity.

I tried with Sir W. Thomson a rough experiment on his yacht with a portable instrument hung up over the side. The orifices of the Pitot tubes could be brought to about four inches from the vessel's skin, and pushed about two feet away from it. We could not perceive that the height of the mercury varied. The vessel was going at the time at the rate of five or six knots. In the Earl of Breadalbane's yacht "Imogine," the nozzles projected 3½ inches, and the height to which the mercury rose was very nearly equal to twice the height due to the velocity, observed, however, from only one run along a mile, shewing, I suppose, that if the speed was correctly ascertained, there was very little disturbance of the water at 3½ inches from the skin at the place of the tubes. Mr. Berthon placed his tubes near the keel, and could project them more or less or withdraw them entirely. They are safer and more simple, however, fixed, for if the sliding ones get bent, they probably become fixed.

It is evident that for such an instrument to be accurate it must be vertical, or the vertical height of the inclined mercury column must be known. It would be easy to hang the instrument in gimbals like a mercurial marine barometer; but I considered that for paddle or screw steamers it is sufficiently accurate, more simple, and less likely to get damaged, to fix it to the ship. When the vessel rolls, the lowest reading of the scale, or when passing the vertical, will be nearly correct. However, when the inclination is considerable, as in sailing yachts or ships, the instrument must either be suspended in gimbals or other means of ascertaining the vertical height found. While looking for other means in designing an instrument for Mr. Jas. Young's yacht, "Nayanza," it occurred to me, that by combining what Euclid and Sir William Thomson had

done, the thing might be managed, and the vertical height of a column of mercury in a ship inclined by waves easily observed without the inclination being known.

Many attempts have been made, by means of the pendulum, to measure these inclinations; but on account of the pendulums which had generally considerable length, swinging on their own account, independently of the ship, no satisfactory results had been obtained by them. The Astronomer Royal for Scotland applied the gyroscope to the purpose and succeeded. The apparatus on the table is one according to his design, which I used on several voyages; but, requiring to be wound up and spun for each set of observa-

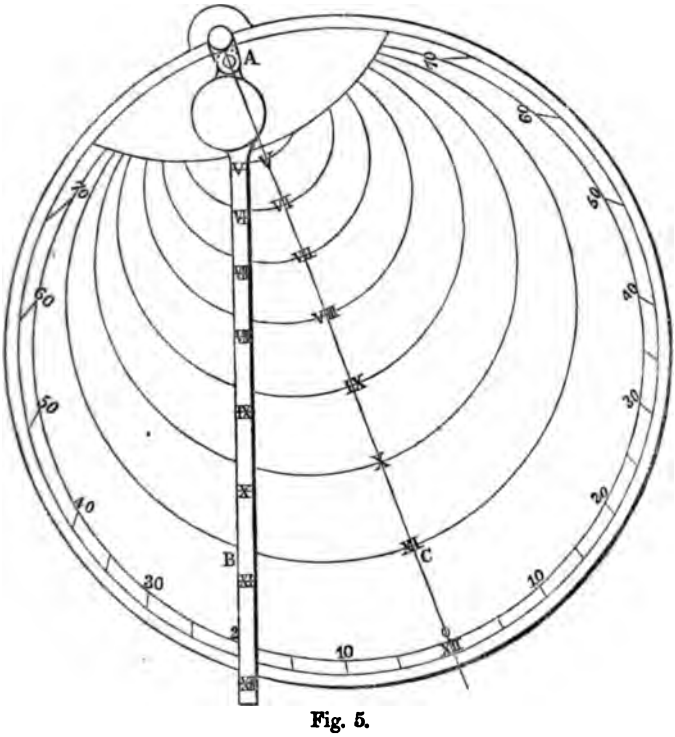


Fig. 5.

tions, it soon ceased to be an interesting occupation even for an idle passenger. Sir Willliam Thomson has fallen back upon the pendulum, which, by making heavy and very short, so that its period of vibration is very quick, virtually moves only as the ship moves, and a long light index-rod attached to it enlarges the scale to what may be wanted. This I have adopted in the instrument

before you (fig. 5). Euclid's share in it is this,—he demonstrated long ago that all the angles in a semicircle are right angles. The curved lines on the dial being circles drawn through the projection of the axis, with their centres in the same straight line, it follows, if the dial is fixed when the ship is upright to a cross bulkhead,

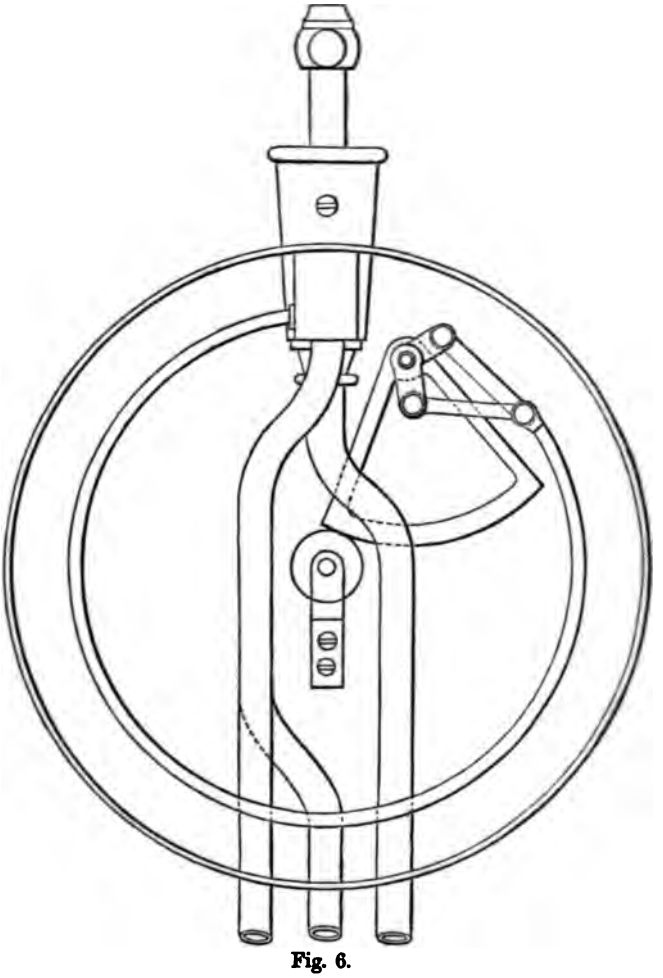


Fig. 6.

with this line vertical, that when the ship rolls or inclines to either side, the length of the pendulum and its continuation along the index-rod, which any of the circles on the dial cut, is the vertical height of the column, whose inclined length is the diameter of the *circle cut;* A B is the vertical height of the inclined column A C.

The circles on the dial are at distances corresponding with the divisions of the Log. The instrument shews, for example, that if the vessel is laid over to about 32°, and the mercury shews 12

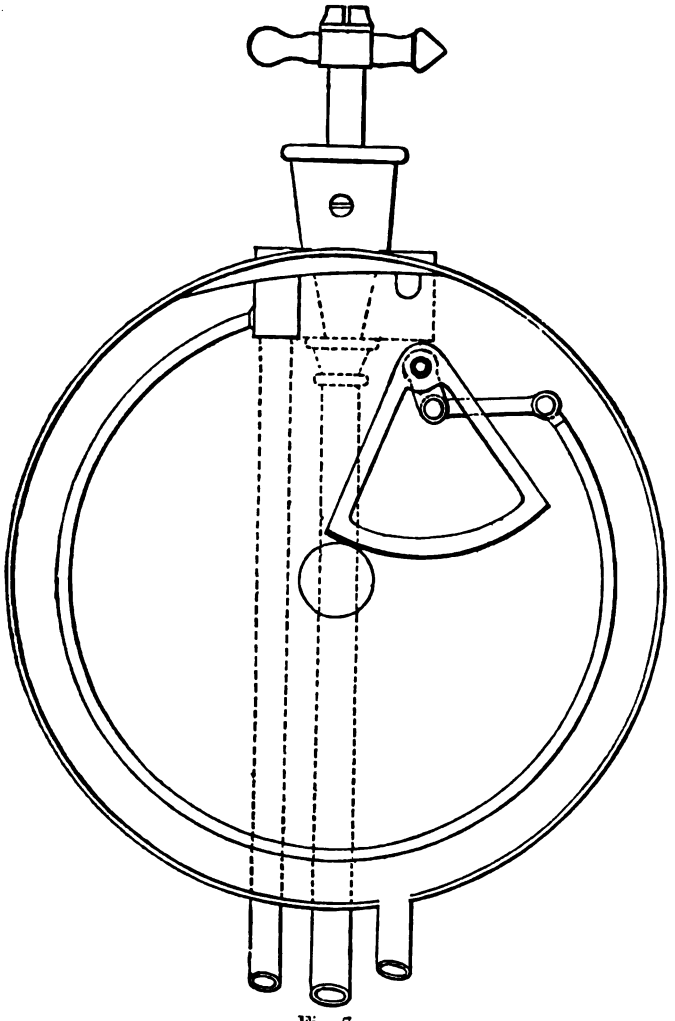


Fig. 7.

knots, the real speed is seen at once to be 11 knots, no calculation is required, and as easily for any other speed or inclination.

Another form into which I have put the Pressure Log dispenses with

the mercury, and I am in hopes will also remove the necessity for a pendulum; but, as it has not been tried, I hesitate to say much about it. It consists in applying the pressure in the Pitot tubes, as I shall continue to call them for simplicity, to an arrangement of Bordon's two-direction-flexible-tubes. The two instruments on the table (figs. 6 and 7 (show their construction. In the one (fig. 6) there are two Bordon tubes, and one of Pitot's tubes is attached to each and to the cock already described. The cock removes the air as in the mercurial log, and the instrument will be graduated as before.

In the other instrument (fig. 7) there is but one Bordon tube, to which one of Pitot's tubes is attached, and the other Pitot tube is attached to the case, with the cock arrangement as before. The case is water-tight, and the index works in water—this is probably the simpler of the two arrangements.

It is difficult to say to what uses such an instrument may be applied, as new tools generally find work for themselves. Besides its ordinary use in navigation as a log, it is evidently valuable for experimental purposes, enabling the resistance of vessels to be calculated with a near approach to accuracy, by giving the velocity of the ship at the instant that the data for ascertaining the power exerted by the steam or wind is obtained. This was the chief use I formerly made of it. I conceive also that it will be found useful to steamers sailing in a fog; for although the normal speed may be known with tolerable accuracy, few know without constantly heaving the common log what the speed is when it is reduced, and therefore become uncertain of their position. In a court of law, in cases of collision, it would not commit perjury. It is, besides, when suitably placed, of the same kind of use in a cabin—to passengers, or to yachtsmen and their friends, as a common clock. And it may conduce to greater safety, and to a saving of time, by enabling the engineer to stop the *ship* if he is required to do so, instead of merely stopping the *engine* as at present.

Before concluding, Mr. Napier shewed an instrument made for H.M.S. "Triumph" clearing itself of air when placed about 4 feet above the water-level—the appliances of the hall not permitting a greater test of its lifting power—and when the cock referred to in his paper was shut, and the experimental water cistern slowly raised, representing a gradual increase of speed in the ship, the mercury in the log was seen to rise to about 6 inches, thereby indicating a speed of about 8¼ knots.

AT the conclusion of the paper, SIR WILLIAM THOMSON said: "The ingenuity Mr. Napier has shewn in overcoming the great difficulties in making the log act satisfactorily in any position above the sea-level is very remarkable. It is indeed an excellent invention, considering that it gives the speed of the ship at every instant. An observation made occasionally in this way will quite suffice to give the distance run. For experiments on sailing vessels it has a very particular merit in giving the speed at every instant, and by the use of this I believe that new results will be worked out as to the effect of differences of trim and of sails. The accuracy and sensibility of the log I can testify to from having seen it in operation. For the higher speeds in steamers, the height of the mercurial column is always within convenient limits. For sailing vessels the mercurial column is not always sufficiently sensitive, but then a water column may be used."

In speaking of the indicator for reading off the true height of the mercurial column when inclined, Sir William Thomson said that he thought it was an extremely interesting way of correcting the indications of the log.

In answer to MR. MAYER, Mr. Napier stated that he had not observed any corrosion in the nozzles, nor was he aware of any sensible corrosion of the iron plating of the ship from the action of the brass nozzles. Also, if any corrosion of the appertures of the nozzles existed, it did not affect the scale of the instrument. If the appertures got stopped, with nozzles conveniently placed, the cock-plug could be removed and a wire passed through the nozzle; if that failed to clear a passage, the nozzle itself could be unscrewed and the hole stopped with a wooden plug till it was cleared; and if parties were too timid to risk that operation, they must want the use of the instrument till they are cleared in dock or otherwise. Permanent stoppage must be rare, as he had not heard of its occurrence, and was only once aware of a temporary one for a few hours, which occurred in the yacht "Nayanza," in a calm last year, and which the return of the breeze and motion of the ship soon cleared.

A vote of thanks was proposed by the CHAIRMAN to Mr. Napier, and heartily accorded.

NOTE, *Received 13th April*, 1872.

In a "Supplement to the Paper on the Mathematical Theory of Stream-lines," by Dr. Rankine—read to the Royal Society on 10th

February, 1870—but only recently published, the author has shewn that the point of no disturbance of the water on the surface of a sphere passing through it is at an angle of 49° 59′, nearly, with the direction of motion—a result agreeing so nearly with the angle 49° 30′ observed by Mr. Berthon, in his Cylindrical Tube, as to induce great confidence in the practical application of the author's Theory of Stream-lines.

JAS. R. NAPIER.

VII.—*On the Results of the Recent Solar Eclipse.* By DR. THORPE.

Read before the Society, 6th March, 1872.

ON the *ne sutor ultra crepidam* principle I feel, in some degree, that I ought to tender an apology for my appearance here to-night. As one of the laity, and with Professor Grant in the chair, I deem that it is in no small degree presumptuous in me to attempt to address you on a subject apparently so far removed from my true vocation. But we chemists are terrible Ishmaelites: our motto is *ubique*. We are continually invading somebody else's preserves; and the last piece of territory we have filched, or rather, let us say, annexed—that's the proper word—is a good slice out of the province of the astronomers. For, I respectfully submit to our President, that he who occupies himself in searching for the vapours of iron and sodium in the atmosphere of the sun is rather more of a chemist than he is of an astronomer. I confess, gentlemen, that the subject of solar eclipse has an especial interest for me; and my coming before you to-night arises from a desire I felt to put together the broad results obtained during the last eclipse, and to compare them with the state of our previous knowledge, in order that we might determine how far a real advance had been made. This I take to be a subject very proper for the consideration of this Society; for the questions which it opens up are of immense importance to us, as bearing directly upon the constitution of our cosmical system.

I doubt not that it will be within the memory of everybody here that on the 12th of last December there occurred a total eclipse of the sun, visible in the southern part of India, and over a large section of Australasia. The great importance of this event, in its relation to

our knowledge of the constitution of the sun, has led to a very careful observance of it; and I purpose to-night to bring under your notice some of the chief points established by the various expeditions which have been fortunate in witnessing the phenomenon. In the outset, I may say that this eclipse has been viewed under exceptionally favourable conditions; but I regret that the time which has subsequently elapsed has not been sufficient for us to come to a definite conclusion respecting many of the vexed questions which it was hoped that the present eclipse would settle. It is true that we are in possession of many capital facts; but the greater number of the reports have yet to be sent in. Still, I think I need offer no apology for bringing the subject of this eclipse under the notice of the Society at this somewhat premature stage. The remarks which I offer may at least serve to direct the attention of some here to the more complete publication of the results which will doubtless occur in our recess.

Let me, in the first place, state the general nature of the phenomena seen during a total eclipse of the sun, in order that we may gain some insight into the character of the problems which it presents. Most of you will be aware that when the sun is eclipsed the dark disc is, among other things, seen to be surrounded by a luminous halo, which we term the corona. Now, it is touching this corona that I wish principally to speak this evening: it is the centre of interest with us at present, and has been the main object of attack during the eclipses of the last few years. You must therefore pardon me if I appear to dwell to an inordinate length upon this particular feature.

Of the chromosphere I shall say little or nothing. That is a portion of the subject which is peculiarly our President's; for, I need not remind the Society that we owe to the critical labours of Professor Grant the discovery of that portion of the luminous envelope which we to-day term the chromosphere.

The corona was noticed so far back as the year 96 by Philostratus; but Kepler has the credit of having first drawn the special attention of astronomers to its existence, and he threw out the idea that it was due to a lunar atmosphere. Kepler conjectured that the solar rays in passing to the earth were refracted in penetrating this hypothetical atmosphere, occasioning the appearance of the luminous ring. This idea was shared by many of the earlier astronomers—for instance, by Halley, one of the most famous of eclipse observers. But I need scarcely tell you that the supposition of a lunar atmosphere of appreciable density is rendered doubtful by a multitude of observa-

tions. I say of appreciable density, for the mathematical reasoning of Clausius has rendered it very probable that our own atmosphere extends even to our satellite, but exists there in a state of extreme tenuity—less, indeed, than 1-700th of the density which it possesses at the surface of the earth. Halley, however, although he seemed to favour it, never definitely committed himself to the explanation of Kepler: it seemed to him incompatible with the fact which he and others before him had detected, that the luminous ring was not concentric with the disc during the progress of the eclipse. This idea of *something* at the moon,—atmosphere, emanation, or call it what you like—occasioning this appearance seems to have retained its hold on the mind of observers for a long time, although the results of each successive eclipse render the probabilities of its existence more and more remote. From the apparent extension of the luminous ring, as seen during the total eclipse of 1806, it has been calculated that the lunar atmosphere, supposing that it existed and produced this effect, would be even more dense than our own. This *reductio ad absurdum* would seem at once to clinch the difficulty; but, unfortunately, as we shall shew, the premises, although they lead to a result consistent with our present information, were not such as to form the basis of a definite calculation. Driven to regard the hypothesis of a lunar atmosphere as untenable, other observers have sought to connect the moon with the phenomenon upon another principle. Delisle, for example, considered that the corona might be due to the diffraction of the solar rays in passing near the moon's edge, and he based his supposition on experimental evidence of no mean weight. This idea was more fully worked out by the late Professor Powell; but although the circumstances of these experiments, considered in the abstract, bear some resemblance to those which occasion the manifestation of the luminous ring, the theory (as Sir David Brewster has proved) is incapable of explaining many of the main features of the appearance of the corona. On no hypothesis, therefore, can the moon be rightly considered as the direct cause of the luminous appearance round the eclipsed sun: the evidence we possess to-day seems perfectly decisive of this point. Having thus disposed of the moon, I hope to your complete satisfaction, and whilst I am upon the question of the early notions of astronomers respecting the physical cause of the luminous ring, I may mention that it has been referred by some to the *zodiacal light;* and these persons have fancied that they have traced in the configuration of the corona an elongation in the direction of the ecliptic exactly corresponding to the appearance of the *zodiacal* light. But a more intimate knowledge of the changes of

this configuration will serve to dispel this idea completely. But I would whisper, lest it disturb the ghosts of those who formerly believed in their relation that the spectroscope has seemingly furnished a grand argument for the connection of corona and zodiacal light, in the fact that the famous green line in the spectrum of the corona (about which more anon) is said by some to be identical with that which Angström has detected in the zodiacal light. I may also state in this connection (you taking it for what it is worth) that Professor Adams and his party saw a very marked display of the zodiacal light in Sicily on the day before the eclipse of December, 1870.

People are not wanting who imagine that the phenomenon of the luminous ring is of purely electrical origin; and Professor Osborne Reynolds exhibited at the Edinburgh meeting of the British Association an experiment designed to illustrate this supposition. He finds that when a ball is electrified in a rarified atmosphere, under a certain relation of electrical tension and atmospheric rarifaction, the ball is surrounded by a luminosity which recals in a very striking manner the general appearance of the corona; but it remains to be seen how far this idea can be brought to harmonize with the revelations of the spectroscope.

But it is not necessary to wait for an absolutely total eclipse to see this corona: this fact, formerly not generally credited, although the observation had been made by perfectly trustworthy observers, has now been incontestibly established. Thus, in the famous eclipse of 1842, Petit, at Montpellier, noticed the ring about six seconds before the sun had totally disappeared behind the dark body of the moon; and his observation was confirmed by Mr. Baily in Italy: nay, the luminous ring has been seen from places outside the limits of the shadow, and where consequently the eclipse was throughout its entire course only partial. Mrs. Airy, who observed the same eclipse, has given us a circumstantial account of the fact. "A bright line," she says, "seemed to form round the right side of the moon before the disappearance, but not quite round, so that the ring was not complete; but at the moment of total disappearance the ends seemed suddenly to join and form the complete ring." To come down to more recent times, M. Rumker, in 1860, observed the corona one and a-half minutes before totality; and Mr. Brett and Professor Watson, in Sicily, in 1870, saw it when a large portion of the sun still remained unobscured. Mr. Lockyer also made the same observation during the last eclipse: he says that for nearly three minutes after totality did the coronal structure impress itself on his retina,

until at last it faded away in the rapidly increasing sunlight. These observations are of great significance in the argument for solar origin; they lend probability to the conjecture that the corona may yet be traced, perhaps, by some process analogous to that by which Lockyer and Jannsen have picked up the chromosphere, by diluting the glare which masks it.

But now, to come to particulars about this corona. Let us inquire more minutely into it. What does it look like? What is its shape? What is its colour? Is it the same thing throughout? And lastly, What is it made up of?

First, then, as to its appearance. This is such that scarcely two observers, although placed side by side, can agree about it. The corona is the very Proteus of celestial phenomena. This fact came out very strikingly in the famous eclipse of 1851, visible in Sweden. Here are some representations of the corona made at that time. First, we have a drawing by Mr. Carrington, in which you see the corona figured as a modest little circle of light, with nothing particularly striking in its appearance; here is a drawing made during the same eclipse by Mr. Airy, presenting a totally different effect from the other: you will notice the "compass-card" ornament surrounding the narrow circle. Here, too, are representations made by the Americans during the eclipse of 1869: you will see what a gorgeous sight Mr. Gilman must have enjoyed: a pyrotechnical display exceeding any 4th of July demonstration. Others of the party were not so fortunate: the corona appeared to Professor Eastman as a far tamer affair. Here is another drawing made also by Professor Eastman in Sicily, in 1870: compare it with this made at the same time and place by Captain Tupman.

I would direct your attention to these outlying portions of the corona or "streamers," as they are called. These are only seen after totality; and they change from side to side, and dance about almost as energetically as the streamers of the Northern Light. It has long been noticed that the corona is of greatest size and brilliancy on the side where the sun has just been covered; but as the eclipse progresses, the brightness and size of this part gradually diminish, and a corresponding increase is seen on the opposite side, where the sun is about to emerge. Maraldi, at the eclipse of 1724, stated that at the commencement of the total immersion the ring appeared broader on the east than on the west; but that at the end of the total immersion it appeared broader on the west side. The same fact was also noticed independently by Rumker and Plantamour *in 1860;* and in three drawings, made in Spain in 1860, by Mr.

Gilman, we have the same effect. In the first drawing, made at the commencement of the eclipse, we have the corona all round the dark disc, but at one side a great bulging out. As the eclipse progressed this bulging was seen to diminish, and a corresponding bulging out was seen to make its appearance on the other side, until, at the middle of totality, the two bulgings to some extent balanced each other, and gave a peculiar quadrangular form to the corona. This is also remarkable in the drawings by Professor Eastman, taken in 1869. But I would have you clearly to understand that it is only the outlying portions of the corona which are thus seen to vary: the luminous appearance immediately surrounding the sun apparently preserves its appearance throughout the entire eclipse. It would seem, therefore, from this, that the corona is complex, and may be split up into at least two distinct portions: a stable portion immediately above the chromosphere, and above that an unstable portion. This, at least, is Mr. Lockyer's view, and it is supported by evidence of considerable weight. Arago noticed the non-uniformity in 1842, and remarked that the internal portion was more constant in shape and brightness than the remote region. Silva also noticed at the same time the effect; and the line of demarcation to him was so pronounced that he actually succeeded in measuring the internal zone, and found it to possess an invariable breadth of 3′. Mr. Airy also detected it, and estimated it to extend to about 4′. Professor Watson likewise remarked the same appearance at Carlentini, in Sicily, in 1870. I have a distinct remembrance of his description of the peculiar configuration of the internal zone: it seemed to be made up of an immense rolling cloud rising up from the body of the sun, and falling over as if it had reached a medium of a density equivalent to its own.

Measurements of the size of the entire corona are, however, by no means so concordant, a proof that to the stable is added an unstable portion. Petit, for example, found for the entire breadth 8′ 45″, Baily estimated it at 16′, and Struve made it out to extend even 25′ from the moon's limb, all at one and the same eclipse. Now, from one point of view, it is fortunate that these determinations were made by men whose skill in measurement no one can impugn: it is a clear proof, from Mr. Lockyer's point of view, of the uncosmical or subjective nature of a portion of the phenomenon. But there are certain difficulties in the way of our accepting this view of the constitution of the corona, one of which appears to me insuperable. It is based upon the examination of the photographs which we possess. I beg to direct

your attention to this very fine photograph, which we owe to the skill of Mr. Brothers; it is one of the fruits of the Sicilian expedition, having been taken at Syracuse in December, 1870. Now, you will notice that the corona here is the very reverse of symmetrical; and I think that no one would have the temerity to assert that the effect here imprinted has not emanated from a truly cosmical source. A careful examination of his negatives has not revealed to Mr. Brothers any structure differing in one portion of the corona from that in another; and he believes that the outlying portions are in physical constitution precisely similar to those nearest the sun. That they have the same actinic effect is evident, or of course they would not have imprinted themselves upon the sensitive plate. We have luckily reaped a perfect harvest of photographs during the last eclipse, and we must patiently wait for their comparison. There is no doubt that it will throw a great light upon this part of the question. But an additional source of perplexity arises when we compare eye-sketches and photographs of the corona. Compare, for example, this sketch of Captain Tupman's, also done at Syracuse, with Mr. Brothers' photograph; the dissimilarity, when you examine the two, is very great. One of the chief features in Mr. Brothers' photograph is the existence of these dark spaces or "rifts," as they are called, in the continuity of the corona; but no trace of this appearance is visible in the sketch, although these rays come out in a very wonderful manner in Mr. Gilman's drawing, which I before shewed you. But, as Mr. Lockyer has already pointed out, "we must remember, in thus bringing eye-sketches and photographs into comparison, that the eye too often in such observations retains a general impression of the whole phenomenon, while the plate records the phenomenon at the time at which it was exposed." Mr. Lockyer, if I understand him rightly, has also added to this an argument of another nature, and one not so free from objection in supposing that the eye and the camera are dealing with two different kinds of light. It has yet to be proved that the light which acts visually here is not capable of also acting chemically. It seems to me incredible that a substance can exist in a certain area of the corona, and by its luminosity make an impression on the retina, and yet be entirely wanting in rays of a refrangibility sufficient to give an impress upon a sensitive plate. The existence of these dark or non-actinic spaces or rifts has, however, been placed beyond all doubt by the photographs taken last December. One of the earliest items of news brought by the telegraph was, that

Mr. Davis at Bekel had again photographed them, and that they even extended down to the moon's limb.

The shapes of the outlying portions as they appeared to the different observers are remarkable for their diversity. This was well seen in the drawings made in 1842. Great jets of light were represented as darting forth from the luminous circle, sometimes on one side of the moon's limb, sometimes on the other. These were occasionally so numerous as apparently to destroy the annular appearance of the corona, giving it the form of a star. On the other hand, some observers saw only one such extension: Otto Struve remarked one immensely long limb extending as far as 4° from the moon's limb. In this last drawing by Professor Eastman, made at Syracuse in December, 1870, you have an example, although by no means an exaggerated one, of this phenomenon. Not only the contour, but sometimes even the structure of the outlying corona is seen to be modified in this anomalous manner, and you get appearances which have been variously described as resembling "hanks of thread in disorder," "gray Maggies' tails," &c. &c. I dare say some of you will remember a copy of an extraordinary drawing by Mr. Pope Hennessy made during the eclipse of 1868, exhibited here by Professor Herschel, giving you the idea that Phœbus's chariot had gone wrong, and that the fiery steeds were lashing out in a most furious manner. And with regard to the *brightness* of the corona, the same discordances are observed. To some it has a lustre resembling that of the moon; to others it appears with far greater splendour; and in the eclipse of 1842 it appeared so intense to Otto Struve as to be barely supportable by the naked eye. There is no doubt, as Arago has pointed out, that these diversities are dependent upon the altitude of the sun at the place of observation, and upon differences in atmospheric conditions. At a low altitude, where the light of the corona is filtered through a comparatively large mass of the atmosphere, it suffers diminution; whereas, when the sun is high in the heavens, the light would be relatively greater. The colour, also, of the corona has been variously stated. To some it is of a peach colour; to others it is pure white. In the short glance I had of it in 1870, it appeared to me perfectly white—of a peculiar pearly lustre, resembling the character of light reflected from the inner surface of a shell. To some the brilliancy of the light appears to *vary during the duration* of the eclipse, sometimes with such suddenness as to give a flickering movement to the ring. Possibly Mr. Gilman attempted to depict such a bizarre effect in the drawing I have shewn

you already. Some persons have said that the ring appeared to turn round on its centre. It has been described as a pleasant example of rotatory motion. One of the officers of the iron-clads, trying to save the poor Psyche, off Aci Reale, having acknowledged that he had had a splendid view of the eclipse, Mr. Lockyer asked him why he had not attempted to depict what he had seen. "How on earth," replied he, "could you attempt to draw a thing which was going round and round like a firework." Even the most qualified observers, such as Mr. Baily and Otto Struve, have observed the same effect. Now, what is the upshot of all this? Simply this, that the corona is, *at least in part*, a purely subjective phenomenon, and that all the changes of colour, of shape, of size, these flickerings, streamers, and what not, are due to our atmosphere, or to physiological differences in the eyes of observers.

It would seem, therefore, on the whole, to be established, that the discordances in size and figure of the corona are due partly to different atmospheric conditions, and partly to the subjective nature of the phenomenon. We have parallel instances of such discordances in the measurements of comets'-tails, as made from different places on the earth's surface. Thus at Paris, as Professor Grant tells us, the comet of 1680 was observed to be 62° long; at Constantinople it appeared to reach to 90°. The tail of the comet of 1769 was 43° long, at London, on the 9th September. On the same day, at Paris, it was 55° long; at the Isle of Bourbon it was 60°; at Teneriffe it was 75°. To come down to more recent times, the tail of the comet of 1843 measured only 40° in England and France; in the tropics it extended over an area of 65°. I should like to ask Professor Grant how far he considers that the determination of the length of a comet's-tail may be influenced by the personal errors of observers? Is any part of the appearance subjective? and if so, to what extent would this affect the determinations of two observers placed side by side? If it could be proved that great differences might occur, it would go a long way to clear up some of our doubts respecting the extension of the corona. I am strengthened in this belief by Kepler's observation, that the tail of the comet of 1607, which at one time appeared short, would in the twinkling of an eye become very long. It was also observed that the tail of the comet of 1608 exhibited a vibratory or undulatory movement, as if the substance of it were blown about by the wind; and in the account of the comet of 1843 it is mentioned that, on a certain night, it darted forth a new tail nearly twice as long as the original one, and forming

with it an angle of 17°. This supplementary tail was not seen on any other night during the visibility of the comet.

Before passing on to the subject of the constitution of the corona, I should like to mention certain curious phenomena seen during the progress of an eclipse, which have certainly some connection with the peculiar changes occurring in the outer regions of the corona. We notice, in the first place, that the colour of the sky undergoes a great change. Halley observed this in 1715:—"When the eclipse," says he, "was about 10 digits (that is, when about five-sixths of the solar diameter were immersed), the face and colour of the sky began to change from perfect serene azure blue to a more dusky livid colour, intermixed with a tinge of purple, and grew darker and darker till the total immersion of the sun." At the same time, the colour of terrestrial objects undergoes an extraordinary variation. Everything has a bright yellow tinge. I shall never forget this effect as I saw it in Sicily in 1870. Beneath my place of observation was a great profusion of prickly pears, growing out of the immense lava stream which had come down from one of Etna's satellites. The broad flat leaves of these plants were admirably adapted to perceive the changes of tint. I noticed that the maximum of yellow occurred about half an hour before totality. At the eclipse I attempted to determine, in conjunction with Dr. Roscoe, the relation between the actinic effect of sunlight and the obscuration of the solar disc, and we found a curious relation to exist between the amount of direct and diffused chemical intensity and this growth and decrease of yellow light. I hope to have the honour of bringing this and other results of the measurement of the chemical action of light under the notice of the Society on a future occasion. Arago has given an explanation of this change in the colour of the sky, and of terrestrial objects: possibly a comparison of our measurements with his conclusions may yield some information. The colours of distant objects, too, at this eclipse were most remarkable. To the north-west of us the horizon was lined with the most gorgeous tints of yellow and green, whilst the hills before us seemed solid masses of ultramarine, and looking eastward over the sea one could trace the streak of bright yellow light outside the limit of totality. The darkness during this eclipse, although we were near the centre of the line, was by no means so profound as I expected it would be. I had no difficulty in reading my pencil-notes, or in seeing the face of my chronometer.

I regret that the time at my disposal will not permit of my

dwelling upon other interesting phenomena seen during an eclipse, —such as the moving shadows which sweep across the earth, the illumination of the moon's surface by light reflected from the earth, the appearance known as Baily's beads, &c. &c. As we are more immediately interested in the corona, it is necessary, before detailing the results of the Indian observations, that we should briefly state the conclusions to which we have arrived up to the present time, respecting its constitution. You all know that it is by means of the spectroscope especially that we seek to establish this. This instrument was first applied to the solution of this question in 1868; and since then spectroscopic observations have become the principal feature in eclipse work. Colonel Tennant reported, in 1868, that the spectrum of the corona was continuous; but this conclusion was not confirmed by Mr. Pogson, although the latter gentleman's observation did not receive its due amount of attention at the time. There is no doubt that Mr. Pogson was the first to observe the existence of self-luminous matter in the corona. The existence of this matter was in the following year confirmed by Professors Harkness and Young, who observed several lines in the spectrum of the corona, particularly a bright green line, corresponding to 1474 on Kirchhoff's scale. The existence of this line was confirmed by Young in Spain, and Harkness in Sicily, in 1870. Everybody is now agreed about its existence; but everybody is not agreed as to what it denotes. Lockyer imagines that it denotes a new element. The line, however, corresponds to one in the spectrum of iron; but the difficulty of believing it to be due to this metal arises from the fact that no other of the 200 lines which have been observed in the spectrum of iron is present in that of the corona. Professor Young has thrown out the rather vague idea that this line in the iron spectrum owes its origin to some foreign substance—standing possibly in some relation to the magnetic properties of the metal. This has evident reference to an idea, also broached by Young, that there is a probable connection between the aurora and the corona—in other words, that the corona is a permanent solar aurora; and, as everybody knows, there is a very singular relation between the manifestation of the aurora and magnetic disturbances. But if the magnetism of iron had anything to do with the appearance of the 1474 line, we ought, I suppose, to observe the same line in the spectra of other metals known to be magnetic, such as nickel and cobalt; but no such coincidences are observable in the spectra of these metals. It appears to me that Mr. Lockyer

has himself supplied us with an experiment which, in spite of the absence of the other 199 lines, may prove that this green line is after all due to iron. He and Dr. Frankland have shewn that the character of a spectrum of a glowing gas varies with the temperature and pressure of that gas. Hydrogen under the normal pressure gives a spectrum characterized by a number of bright lines; but when the gas is excessively rarified, this number dwindles down to one, and the other lines give place to a continuous spectrum. This, too (for hydrogen is undoubtedly one of the constituents of some portion of the corona), accounts for the continuous spectrum observed by Tennant, the lines near E., F., and D., observed by Pogson, Young, Harkness, and others. These observers were evidently observing different regions of the corona—regions in which the glowing gas was at very different temperatures.

And now we come to the results of the Indian eclipse of last December. My information of these is mainly derived from the observations of Mr. Lockyer, Commander Maclear, and Professor Respighi. This diagram shews you the path of the moon's shadow. Messrs. Lockyer and Maclear observed it at Bekul, on the west coast, and Professor Respighi at Poodocottah, near the east coast. In the eclipse of 1870, Professor Young had observed that, just before and just after totality, the dark lines in the solar spectrum gave place to bright lines. This phenomenon was carefully watched for by Mr. Lockyer; but it was not seen, nor did any of the observers stationed with him perceive it. Mr. Lockyer has described a naked-eye observation of the corona:—"It was of the purest silvery whiteness, with a star-like decoration, with its rays arranged almost symmetrically—three above and three below—two dark spaces or rifts at the extremities of a horizontal diameter. The rays were built up of innumerable bright lines of different lengths, with more or less dark spaces between. Near the sun this structure was lost in the brightness of the central ring." The examination of the spectrum of a streamer shewed that it was due to glowing hydrogen, 1474 rather faint and thickening downwards; the line C and the continuous portion of the spectrum were exceedingly vivid. The spectrum was then examined without a slit—that is, through a simple train of prisms,—a new feature in the observations, and suggested independently by Messrs. Lockyer and Young,—"Its principle being," to use the former gentleman's words, "that, in the case of particular rays given out by such a thing as the chromosphere, or the sodium vapours of a candle, we shall get images of the thing itself painted in that part of the

spectrum which the ray inhabits. So to speak, we shall see an image for each ray, as if the prisms were not there." What Mr. Lockyer saw was, "Four exquisite rings, with projections where the prominences were. In brightness, C came first, then F, then G, and last of all 1474. Further, the rings were nearly all the same thickness, certainly not more than 2′ high, and they were all enveloped in a line of impure continuous spectrum." The structure of the corona was exquisitely and strongly developed, reminding one of Orion. "Thousands of interlacing filaments," says Mr. Lockyer, "varying in intensity, were visible; in fact, I saw an extension of the prominent structure in cooler material. This died out somewhat suddenly some 5′ or 6′ from the sun. I could not determine the height precisely, and then there was nothing; the rays so definite to the eye, had, I supposed, been drawn into nothingness by the power of the telescope; but the great fact was this, that close to the sun, and even for 5′ or 6′ away from the sun, there was nothing like a ray, or any trace of radial structure to be seen." To Commander Maclear the corona appeared as a bright glory round the moon, stellar form, six-pointed, something like the mitres painted round a saint's head, extending to a diameter and a half. On looking through the finder, the same form was seen, but much reduced in size and brilliancy; its colour appeared to be a light pinkish white, very brilliant, and it had no streamers. Mr. Pringle, also observing at Bekul, remarked that the corona appeared radial, of a purplish white colour, brightest near the body of the moon, and no very long rays perceptible. Appearances were not altered by holding the head sideways. The structure, as observed through the finder, was well defined and wavy; it extended to about 7′ beyond the sun. The photographs at Bekul were very successful, shewing several rifts and extensive corona, with slight external changes. Even more marked changes were recorded by the sketchers, especially in the outer regions of the corona.

Professor Respighi, well known for his researches on the chromosphere and prominences, observed at Poodocottah. The problems which he set himself to determine were—

1. To ascertain whether, just before the beginning and at the end of totality, the solar spectral lines were reversed.

2. Amidst the several chromatic images of the prominences, to observe especially whether the image given by the yellow line D_3 coincides with that of the lines of hydrogen gas.

3. To define the form and dimensions of the chromatic images of the corona.

The day before the eclipse, Professor Respighi delineated the profile of the solar disc, in order to ascertain the state of the chromosphere at the several parts of the limb, and the protuberances existing there. The picture shewed that, both on the eastern and western limb, at the point where contact would take place, the chromosphere was in that abnormal condition which is generally observed in the neighbourhood of solar spots. The number of the prominences was, however, small, and their dimensions moderate—conditions favourable to the examination of the corona. On the day of the eclipse, the first observation made had reference to the existence of Young's bright line stratum; but Professor Respighi failed to perceive it.

Of the work done by the other observers I have but a meagre account to offer: their reports have yet to come in. Colonel Tennant's party at Ootacamund, midway between Bekul and Poodocottah, where you will remember Mr. Lockyer and Professor Respighi observed, were also very successful—Colonel Tennant observing a continuous spectrum with 1474. The photographs were also good in the main, agreeing, so far as the contour of the corona was observed, with those taken by Mr. Davis at Bekul. Dr. Jannsen (who is memorably connected with Indian eclipses) was favoured with a sky of almost absolute purity. The spectrum of the corona appeared discontinuous, containing lines due to hydrogen, the brilliant green line of Young and Harkness, D, and some others more feeble. The parties in Ceylon were also successful, as the morning was clear and bright. The results, in the main, are identical with those of the other parties, excepting that the reversal of the dark lines was at one place (at Jaffna) observed.

But I fear I have already trespassed too long on your patience: I find that I have exceeded the time to which I had limited myself. In concluding, I would simply state what I think must be apparent to you all—that the results of the Indian eclipse of 1871 are in the highest degree important and satisfactory. The composition and structure of at least a part of the corona is absolutely settled; and a comparison of the seventeen photographs, taken by instruments of the same power and pattern—eleven of them taken four hundred miles apart, and six at an intermediate and elevated station—will doubtless go far to trace the influence of our atmosphere on the phenomenon. There are certainly discordances in the observations; but still, when we remember how beautifully former discordances even more contradictory have been brought together in harmony, these disagreements may turn out to be as valuable as the coinci-

dences. We are still to some extent groping in the dark; but the eclipse of 1871 has thrown a great ray of light on many of the obscure points of our knowledge of the constitution of our great luminary; and, when its results are collected, arranged, and discussed, I doubt not that the 12th of December, 1871, will be a red-letter day in the history of solar physics.

On the Analysis of Chrome Ore. By JOHN CLARK, Ph.D.

Read before the Chemical Section, 4th December, 1871.

IN 1862 Dr. Genth called attention to the discrepancies between his own and other chemists' analyses of chrome ore; and even at the present day there are few analyses in which, generally speaking, the results of chemists disagree to a more marked extent. In addition to the difficulty of obtaining concordant results by different chemists, the consumers of chrome ore complain that in the manufacture of bichromate of potash there is a loss of oxide of chromium which they can only account for on the supposition that the chemist generally reports too high a result.

These complaints do not appear to be altogether unfounded. Undoubtedly, many discrepancies are due to differences in the sample, but still more so to the inexperience of the analysts, and the difficulty of obtaining correct results by the ordinary methods of analysis. The impossibility of obtaining, by washing alone, an oxide of chromium precipitate perfectly free from alkali, after fusing the ore with bisulphate of potash, was recognized by Dr. Genth, who recommended that, before weighing, the ignited precipitate should be boiled with water containing sulphurous acid, to reduce the chromate which had been formed, and that the oxide produced by the reduction should be precipitated by ammonia. By this means the weight of the precipitate of oxide of chromium is considerably reduced, but, in the experiments I have made, never quite pure. Instead of boiling the ignited precipitate with water containing sulphurous acid, it may be boiled with dilute acetic acid, and the chromic acid, thus removed, estimated with a dilute solution of double sulphate of iron and ammonia of known strength. The oxide of chromium, in the form of chromate, usually amounts, *even in* the most carefully washed precipitate, to 0·76 per cent.

Perfectly accurate results by the precipitation method, without subsequent correction, can, I believe, only be obtained in the absence of sulphate; as, for example, by the process recommended by M. Clouet, manufacturer of bichrome in Havre (see *Annales de Chemie et Phys.*, 4 series, t. xvi. p. 90). M. Clouet's process consists in fusing the finely pulverized chrome ore with five or six times its own weight of pure carbonate of soda, in a platinum crucible, for five or six hours, at a bright red heat, without the addition of any oxidizing agent, reducing the purified chromic acid formed with alcohol, in presence of hydrochloric acid, and precipitating the oxide of chromium by ammonia. In this case, owing to the comparatively small amount of salts present, and the absence of sulphates, a perfectly pure oxide of chromium precipitate may be obtained. Although M. Clouet's method yields the correct amount of oxide of chromium, it is not well adapted for the laboratory, on account of the length of time required for the complete oxidation of the oxide of chromium in the ore.

Perfectly accurate results may be obtained by the following volumetric method, which is at once simple and expeditious :—

Twenty-five grains of the finely powdered ore are intimately mixed with two hundred grains of hydrate of sodium and calcined magnesia, in the proportion of five parts of hydrate of sodium to three of magnesia, and the mixture exposed in a platinum crucible to the heat of an ordinary Bunsen burner for three quarters of an hour, with the lid off, and afterwards for a quarter of an hour with the lid on. Under the influence of the hydrate of sodium and magnesia oxidation takes place at once, and is practically complete in less than an hour. The crucible with its contents is then placed in a basin containing water; the contents are washed out as far as possible with water, and afterwards with dilute sulphuric acid, which must be free from nitric acid. An additional quantity of pure dilute sulphuric acid is then added to the contents of the basin and heat applied, when everything should dissolve except, perhaps, a few flakes of silica. To the clear yellow solution is added a weighed quantity of double sulphate of iron and ammonia, of known strength, and more than sufficient for the reduction of the chromic acid. The excess of unoxidized iron is then estimated with a weak standard solution of bichromate of potash, and the amount of oxide of chromium in the ore calculated from the quantity of iron oxidized. It not unfrequently happens that a little of the chrome ore escapes decomposition: this must be filtered off after the chromic acid in solution has been estimated, and again fused with

hydrate of sodium and magnesia as before, as it is generally richer in oxide of chromium than the original ore. It may readily be supposed that soda-lime, or a mixture of hydrate of sodium and lime, would induce oxidation quite as readily as a mixture of hydrate of sodium and magnesia; but this is not the case. When chrome ore is heated over an ordinary Bunsen burner with soda-lime—hydrate of sodium and lime—or even with a mixture of hydrate of sodium and magnesia, which has been ignited before admixture with the ore, only a trifling oxidation takes place.

The results obtained by the process which I have described seldom vary 0·2 per cent. I have had occasion to submit this process to several chemists; among others to the director of the school of mines in St. Petersburg, who has not only expressed a very favourable opinion of its accuracy, but, as I understand, adopted it in the testing of the Russian ores.

The other ingredients usually existing in chrome ore (viz., oxide of iron, alumina, magnesia, and silica) may be estimated by the process described by Fresenius. I consider, however, that M. Clouet's method of estimating the iron, as described in the paper to which I have referred, must give incorrect results; and I direct attention to this more especially on account of the formulæ which that gentleman has given to express the various proportions in which the oxide of iron and oxide of chromium exist. After fusing the ore with carbonate of sodium, M. Clouet assumes that all the alumina is soluble in water (as aluminate of sodium), and that the residue, when the ore is decomposed, consists only of oxide of iron, magnesia, and silica; so that when the residue is dissolved in hydrochloric acid, evaporated to dryness, to render the silica insoluble, and filtered, the precipitate produced by the addition of ammonia to the filtered liquid will be pure oxide of iron, which may be collected, washed, dried, ignited, and weighed. This, so far as I understand from his paper, is the method which M. Clouet employed in estimating the iron, although he mentions that the iron might also be estimated by a standard solution of permanganate of potash. By this method of analysis, M. Clouet finds that all the varieties of chrome ore he has analyzed have a definite chemical composition, and contain the oxide of chromium and oxide of iron in definite proportions, after deducting the silica, alumina, and magnesia, which he regards as the matrix of the true ore.

In a number of analyses which I have made by M. Clouet's method, I was never able to render the alumina completely soluble *in* water by fusion with carbonate of sodium, but always found a

considerable portion of the alumina in the precipitate, which M. Clouet regards as pure oxide of iron; and in numerous analyses of chrome ore which I have made, very few of them would be represented by any of M. Clouet's formulæ. Unfortunately, I am not aware of the localities from which my samples were derived; but I do not think it will be out of place to mention the average results which I obtained.

In seventeen analyses of ores, containing from 30 to 40 per cent. of oxide of chromium, the average amount of protoxide of iron was 15·4 per cent.

In twenty-three analyses of ores, containing from 40 to 50 per cent. oxide of chromium, the average amount of protoxide of iron was 14·3 per cent.

In thirteen analyses of ores, containing from 50 to 60 per cent. oxide of chromium, the average amount of protoxide of iron was 15·0 per cent. (the lowest amount of oxide of iron was 10·6, and the highest 23·14 per cent.)

In each case the iron was estimated in the ammonia precipitate by a standard solution of bichromate of potash, after reduction with Stannou's chloride.

I am not prepared to say that the protoxide of iron and oxide of chromium do not exist in definite proportion; but if so, then other formulæ in addition to those given by M. Clouet will be required to express their composition.

IX.—*On some Experiments with Fehling's Copper Solution.*

By T. L. PATTERSON, F.C.S.

Read before the Chemical Section, 11th March, 1872.

THE copper solution used in the following experiments were prepared according to the instructions given by Fresenius.

34·632 grms. of sulphate of copper, dissolved in 200 c.c. of water, were added to a solution of 173 grms. tartrate of soda and potash, in 480 c.c. of soda solution of 1·14 sp. gr., or thereby, and the whole made up to one litre. They were tested with one of inverted sugar prepared as follows:—·5 grm. of pure dry loaf sugar, free from glucose, was dissolved in a small flask, in 10 c.c. of water, 30 drops

of hydrochloric acid added, and a thermometer inserted. The flask with its contents was then placed in a water bath for fifteen minutes, or until the thermometer indicated 70° C., when it was removed and diluted to 300 c.c. Each c.c. of this solution was thus equal to ·001754 grm.

The experiments were conducted in flasks, rather than basins or beakers, because with the latter, that portion above the liquid is liable to become overheated to such an extent, that, when the solution is stirred or shaken, part of it is likely to become reduced, and thus lead to erroneous results. Flasks are not liable to the same objection, if they be occasionally shaken until they boil. When any portion is thus reduced, the whole must be rejected.

Experiment 1 (*a*).—10 c.c. of a copper solution were boiled with about 40 c.c. of water, in a flask, without precipitation. The standard sugar solution was then added from a burette, graduated into $\frac{1}{5}$ c.c., until, on filtering off a portion rapidly, no copper-coloured precipitate was obtained, on the addition of a few drops of acetic acid, and one drop of a very dilute solution of ferocyanide of potassium. 30 c.c. were thus consumed. Another experiment gave the same result; and 30 × ·001754 = ·05262 grm. glucose—the amount required to reduce 10 c.c. copper solution.

(*b*) 10 c.c. of the same solution were treated precisely as in (*a*), with the addition of 10 c.c. of soda solution of sp. gr. 1·163. It now required 33 c.c. of the sugar solution for complete reduction, which is = ·05788 grm. glucose.

(*c*) Other 10 c.c. were boiled with the addition of 50 c.c. of soda solution. When 34 c.c. of sugar solution had been consumed, the cupric oxide was not all reduced. Nor was reduction complete after the further addition of 14 c.c. in quantities of 2 c.c. at a time, testing after each addition. The result was the same when 50 c.c. were added to another portion of 10 c.c., in quantities of 5 c.c., after the 35th c.c.

The results of this experiment lead me to question the conclusions of Fehling, Fresenius, and others, who assume that 1 equivalent of grape sugar reduces 10 equivalents of cupric oxide. Had this been the case, 10 c.c. of the solution in (*a*) would have consumed 28·5 c.c. of the standard sugar solution = ·05 grm. glucose: whereas, 30 c.c. = ·05262 grm. were required. (*b*) and (*c*), I believe, shew the reason of this, and prove satisfactorily that the quantity of cupric oxide reduced, depends as well upon the alkalinity of the solution as on the amount of oxide of copper which it contains. Consequently, as the amount of soda varies, the solution's power of

oxidizing grape sugar will also vary, and must be determined for each solution separately. When, moreover, the soda is in great excess, as in (*c*), accurate results cannot be obtained.

Experiment 2.—With the view of shewing the effect of soda on an old solution, the following experiments were made:—

(*d*) A copper solution, 10 c.c. of which were = ·05262 grm. grape sugar, had been in occasional use for about five months. For some time soda had to be added to prevent precipitation. 10 c.c. were found to = ·039 grm. glucose; but with the addition of 40 c.c., or so, of soda, 10 c.c. were = ·05614 grm. glucose.

(*e*) Another solution having the same sugar-destroying power as (*d*), at the end of five months precipitated on boiling. 10 c.c. were = ·04035 grm. grape sugar. 10 c.c. + 10 c.c. of soda solution, of 1·13 sp. gr., and 30 c.c. or so of water, did not precipitate on boiling; and consumed sugar solution = ·05331 grm. glucose. And 10 c.c. + an addition of 50 c.c. of soda solution, required ·05702 grm. inverted sugar for complete reduction.

We learn from this experiment that soda renews an old solution, as is well known; but the alkali must be added sparingly—in fact, not more than is necessary to prevent precipitation on boiling—otherwise it will have too high a glucose destroying power, and so give a low percentage of fruit sugar in any sample of sugar under examination. We see here, also, as in experiment 1, that the oxidizing power of the cupric solution increases as the quantity of alkali increases, so that, by the addition of soda in excess, an old solution can be made to represent a greater amount of glucose than the same solution did when new.

The deterioration of copper solutions kept for some time, such as those in experiment 2, has been variously attributed to the action of light, absorption of carbonic acid, &c. In Watts' *Dictionary* we are told to preserve the solution in well closed vessels, to protect it from carbonic acid and air. But even when so protected and kept in the dark, a solution in constant use still deteriorates. To what then is this deterioration due? I have always found, on removing the stopper from a cupric solution bottle which has not been opened for a few days, that a partial vacuum existed within, and the vacuum was the greater the longer the bottle remained closed. Clearly this vacuum is not due to the absorption of the small amount of carbonic acid in the air of the bottle: it must, therefore, be owing to the absorption of oxygen; and this I find to be the case—for, on inserting a lighted splinter into a bottle half filled with the solution, it is at once extinguished,

nor can it be made to enter beyond the neck—a sufficient proof that the gas is nitrogen.

Experiment 3.—To determine the amount of this absorption, whether it was partial or complete, the following experiment was made:—Into a tube closed at one end, containing 50 c.c. and graduated into $\frac{1}{5}$ c.c., I inclosed a quantity of dry air over mercury, which, after reduction to 0° C. and 760 m.m. pressure, measured 42·76 c.c. 4 c.c. copper solution—10 c.c. of which = ·05175 grm. glucose—were then introduced, and the whole set aside in diffuse daylight. The experiment lasted about eight months. Absorption was slow and gradual, extending over a considerable period; but not so long as this experiment would lead one to believe. It was only allowed to stand so long in order to be quite certain that absorption had ceased.

The copper solution in the tube had now a much lighter colour, and cuprous oxide was deposited on the sides for the space of about a c.c., where the mercury first came in contact with the solution. The reading of the tube, after making all the necessary corrections, was 34·09 c.c. @ 0° C. and 760 m.m. barometer. Now, 42·76 – 34·09 = 8·67 c.c. oxygen absorbed by the 4 c.c. of copper solution; and $\frac{8·67}{42·76} \times 100 = 20·28$ % of the original air. This agrees very well with the quantity of oxygen in the air, as determined by Bunsen—viz., 20·93 %, and shews that all the oxygen has been absorbed. The difference is due to experimental error; for instance, the barometer used was an aneroid, which always shewed a slight variation when compared with a standard. The residual nitrogen was tested as before, but it would not support combustion.

The foregoing experiment shews satisfactorily the complete absorption of oxygen by the liquid; and in order to observe further the effect of oxidation and decomposition, another experiment was made.

Experiment 4 (*f*).—The copper solution used in this experiment had a glucose destroying power of ·04965 grm. per 10 c.c. The carbonic acid and soda were estimated in separate portions of 10 c.c. each—the former in a carbonic acid apparatus, and the latter by saturation with normal solution of sulphuric acid, boiling to expel carbonic acid, and backward titration with normal soda solution. The copper solution was thus found to contain ·18 grm. carbonic acid, and to have a neutralizing power of 5·39 grm. soda per 100 c.c.

Having been in almost daily use for a month—during which time it was kept in a cupboard—the solution was nearly finished. There

was still no precipitation on boiling. 100 c.c. contained ·30 grm. carbonic acid, and had a neutralizing power of 5·26 grm. soda.

Two small bottles, (*g*) and (*h*), were half filled with solution (*f*), and securely stoppered. (*h*) was placed in the direct sun's rays, and (*g*) was rolled in paper and placed in a tin case, in order thoroughly to exclude light. After the lapse of eight months the bottles were opened and analyzed. (*g*), that bottle kept in the dark, had not changed in depth of colour, no precipitate had formed; it remained perfectly clear on boiling, and contained ·18 grm. carbonic acid, with a neutralizing power of 5·27 grms. soda per 100 c.c. (*h*), the second bottle, exposed in the direct solar rays, was much lighter in colour, the stopper was with difficulty removed, and there was deposited at the bottom a copious crystalline precipitate of cuprous oxide. The solution, filtered from this precipitate, gave a second precipitate on boiling, and 10 c.c. were = ·00087 grm. glucose. It contained ·33 grm. carbonic acid, and had a neutralizing power of 4·89 grms. soda per 100 c.c.

The injurious effect of light on this solution is very well seen in (*h*), nearly all the copper has been precipitated, while the solutions (*f*), at the end of one month, and (*g*) are quite unaffected by boiling; (*h*) has also increased ·15 grm. carbonic acid, and decreased ·5 grm. soda, in neutralizing power, per 100 c.c. Now, this gain in carbonic acid could not have been the result of absorption from the atmosphere, for the carbonic acid had not increased in the bottle kept in the dark (*g*), and the partial vacuum in the bottle shewed that the stopper was perfectly tight. Nor could it have been produced by oxidation, as the bottle in which the solution was contained held about 100 c.c., and was half-filled with the solution, the other half being air; 50 c.c. of air weighing ·0608 grm., and containing ·014 grm. oxygen, are only = ·01924 grm. carbonic acid, whereas ·075 grm.—nearly four times that quantity—was found. Therefore, the carbonic acid found in this experiment is partially produced by oxidation, and partially the result of the decomposition of the tartaric acid promoted by the actinic rays.

The fixation of ·50 grm. soda is conclusive on this point. The tartaric acid, under the influence of light and absorbed oxygen, becomes split up in such a manner that its neutralizing power is considerably increased in presence of the free alkali. We may suppose one molecule of the bibasic acid to decompose into one of another bibasic, and one of a monobasic acid, besides carbonic acid; and so exert an extra neutralizing power on the soda to that extent. I made no experiments to determine what acids these are,

nor can it be made to enter beyond the neck—a sufficient proof that the gas is nitrogen.

Experiment 3.—To determine the amount of this absorption, whether it was partial or complete, the following experiment was made:—Into a tube closed at one end, containing 50 c.c. and graduated into $\frac{1}{5}$ c.c., I inclosed a quantity of dry air over mercury, which, after reduction to 0° C. and 760 m.m. pressure, measured 42·76 c.c. 4 c.c. copper solution—10 c.c. of which = ·05175 grm. glucose—were then introduced, and the whole set aside in diffuse daylight. The experiment lasted about eight months. Absorption was slow and gradual, extending over a considerable period; but not so long as this experiment would lead one to believe. It was only allowed to stand so long in order to be quite certain that absorption had ceased.

The copper solution in the tube had now a much lighter colour, and cuprous oxide was deposited on the sides for the space of about a.c.c., where the mercury first came in contact with the solution. The reading of the tube, after making all the necessary corrections, was 34·09 c.c. @ 0° C. and 760 m.m. barometer. Now, 42·76 – 34·09 = 8·67 c.c. oxygen absorbed by the 4 c.c. of copper solution; and $\frac{8·67}{42·76} \times 100 = 20·28$ % of the original air. This agrees very well with the quantity of oxygen in the air, as determined by Bunsen—viz., 20·93 %, and shews that all the oxygen has been absorbed. The difference is due to experimental error; for instance, the barometer used was an aneroid, which always shewed a slight variation when compared with a standard. The residual nitrogen was tested as before, but it would not support combustion.

The foregoing experiment shews satisfactorily the complete absorption of oxygen by the liquid; and in order to observe further the effect of oxidation and decomposition, another experiment was made.

Experiment 4 (*f*).—The copper solution used in this experiment had a glucose destroying power of ·04965 grm. per 10 c.c. The carbonic acid and soda were estimated in separate portions of 10 c.c. each—the former in a carbonic acid apparatus, and the latter by saturation with normal solution of sulphuric acid, boiling to expel carbonic acid, and backward titration with normal soda solution. The copper solution was thus found to contain ·18 grm. carbonic acid, and to have a neutralizing power of 5·39 grm. soda per 100 c.c.

Having been in almost daily use for a month—during which time it was kept in a cupboard—the solution was nearly finished. There

was still no precipitation on boiling. 100 c.c. contained ·30 grm. carbonic acid, and had a neutralizing power of 5·26 grm. soda.

Two small bottles, (*g*) and (*h*), were half filled with solution (*f*), and securely stoppered. (*h*) was placed in the direct sun's rays, and (*g*) was rolled in paper and placed in a tin case, in order thoroughly to exclude light. After the lapse of eight months the bottles were opened and analyzed. (*g*), that bottle kept in the dark, had not changed in depth of colour, no precipitate had formed; it remained perfectly clear on boiling, and contained ·18 grm. carbonic acid, with a neutralizing power of 5·27 grms. soda per 100 c.c. (*h*), the second bottle, exposed in the direct solar rays, was much lighter in colour, the stopper was with difficulty removed, and there was deposited at the bottom a copious crystalline precipitate of cuprous oxide. The solution, filtered from this precipitate, gave a second precipitate on boiling, and 10 c.c. were = ·00087 grm. glucose. It contained ·33 grm. carbonic acid, and had a neutralizing power of 4·89 grms. soda per 100 c.c.

The injurious effect of light on this solution is very well seen in (*h*), nearly all the copper has been precipitated, while the solutions (*f*), at the end of one month, and (*g*) are quite unaffected by boiling; (*h*) has also increased ·15 grm. carbonic acid, and decreased ·5 grm. soda, in neutralizing power, per 100 c.c. Now, this gain in carbonic acid could not have been the result of absorption from the atmosphere, for the carbonic acid had not increased in the bottle kept in the dark (*g*), and the partial vacuum in the bottle shewed that the stopper was perfectly tight. Nor could it have been produced by oxidation, as the bottle in which the solution was contained held about 100 c.c., and was half-filled with the solution, the other half being air; 50 c.c. of air weighing ·0608 grm., and containing ·014 grm. oxygen, are only = ·01924 grm. carbonic acid, whereas ·075 grm.—nearly four times that quantity—was found. Therefore, the carbonic acid found in this experiment is partially produced by oxidation, and partially the result of the decomposition of the tartaric acid promoted by the actinic rays.

The fixation of ·50 grm. soda is conclusive on this point. The tartaric acid, under the influence of light and absorbed oxygen, becomes split up in such a manner that its neutralizing power is considerably increased in presence of the free alkali. We may suppose one molecule of the bibasic acid to decompose into one of another bibasic, and one of a monobasic acid, besides carbonic acid; and so exert an extra neutralizing power on the soda to that extent. I made no experiments to determine what acids these are,

but they are likely to be related to oxalic, and contain less carbon than tartaric acid.

At the end of one month (*f*) had gained ·12 grm. carbonic acid, and lost in alkalinity ·13 grm. soda. This decomposition is similar to that of (*h*), but for want of time and exposure to light, has not proceeded to the same extent. From the fact, however, of this bottle being in constant use—not under precise conditions, but kept principally in the dark, though occasionally in the light—now open, and again closed—I will not hazard an opinion on the results.

The carbonic acid in (*g*)—the bottle kept in the dark—was the same at the end as at the commencement of the experiment, but the alkalinity of the solution had decreased to the extent of ·12 grm. soda per 100 c.c.; so that although, when kept in the dark in well stoppered bottles, the solution does not oxidize, yet it undergoes a slight internal decomposition—similar to that induced by light in (*h*) —whereby the fixed acidity is increased in presence of the excess of alkali. Light is thus necessary for oxidation, and darkness retards decomposition—phenomena which, I venture to assert, are more general than at first sight might be supposed. With the exception of the decrease in alkali, the (*g*) solution may be said to be as good and sound as the original solution (*f*), proving that it can be preserved under exceptional conditions for a reasonable length of time (eight months in this case).

In experiment 2, (*d*) and (*e*), I have shewn the deteriorating influence of age on solutions in constant use. For the purpose of these experiments my solutions have been kept in the dark. But I have been in the habit of using bottles for preserving the solution, made by the York Glass Co. from a kind of dark green glass. These bottles cut off the actinic end of the spectrum as far as the green, and are useful for preserving nitric acid, silver, and other salts, decomposed by light.

To test the preservative qualities of this glass for the copper solution, I made another experiment as follows:—

One of the green bottles was half filled with the solution, and placed in the direct rays of the sun for six months. At the commencement of the experiment, 10 c.c. were = ·05179 grm. grape sugar; at the end, 10 c.c. = ·05000 grm., and the solution when boiled shewed no sign of precipitation. The result is thus in favour of these bottles; for, with this severe test, the solution has only lost the power of oxidizing ·00179 grm. glucose; while that kept in a white glass bottle under the same circumstances, (*h*), had nearly all its copper precipitated.

Did Fehling's solution only deteriorate by the absorption of oxygen, there might be inserted in the bottle a perforated cork, through which a tube passes holding fragments of pumice moistened with a strong solution of pyrogallate of potash. I have not tried this arrangement; but as I have shewn in experiment 4, (*g*) and (*h*), that decomposition takes place without the absorption of oxygen, I do not think any useful purpose would be served by such an addition.

In conclusion, these experiments shew:—

First, That Fehling's solution has no definite grape sugar equivalent, depending upon the weight of cupric oxide it contains; but that its glucose destroying power varies directly (within certain limits) with the quantity of alkali present, and that when soda is in great excess, all the copper cannot be precipitated with a solution of inverted sugar. (*Experiment* 1, *a*, *b*, *c*.)

Second, That old solutions are renewed by an addition of soda, as our text books set forth. And, moreover, that by adding the alkali in considerable excess, the glucose-destroying power of the solution can be made to exceed that of the same solution when first prepared. (*Experiment* 2, *d*, *e*.)

Third, That all the oxygen is absorbed by the solution from air in a confined space, as in a partially filled bottle. (*Experiment* 3.)

Fourth, That, under the influence of light, decomposition is almost complete, and carbonic acid increases to a greater extent than can be accounted for by the absorption of oxygen. (*Experiment* 4, *h*.)

Fifth, That the amount of soda in the solution decreases with its age, even when kept in total darkness, and that this loss of alkalinity is to be attributed to the decomposition of the organic acid into other fixed acids, the total acidity of which is greater than that of the original tartaric acid. (*Experiment* 4, *f*, *g*, and *h*.)

Sixth, That a solution kept for eight months away from light, in a bottle containing air, is in good condition at the end of that time. (*Experiment* 4, *g*.) And,

Seventh, That the solution is best kept for use in dark-green bottles, which exclude the actinic rays. (*Experiment* 5.)

X.—*On a Double Sulphide of Gold and Silver.* By M. M. Pattison Muir, F.C.S.

Read before the Chemical Section, 15th April, 1872.

This new sulphide was formed by the action of molten sulphur upon a mixture of gold and silver in a fused state.

It is a dark-gray crystalline substance—brittle—decomposed by heating in a current of hydrogen. Fused with sodium carbonate, it gives sodium sulphide, along with a seeming alloy of gold and silver. The following are the results of the analyses, and also the per-centage composition calculated for the formula,—2 (Au^2S^3) 5 (Ag^2S):—

	Calculated.	Found.
S, ...	15·85	16·17
Ag, ...	48·68	49·27
Au, ...	35·47	35·69
	100·00	101·13

The specific gravity of this substance is 8·159.

XI.—*On the Action of Dilute Saline Solutions upon Lead.* By M. M. Pattison Muir, F.C.S.

In this communication I have pointed out the vagueness of the statements generally found in the text-books, as to the amounts of lead dissolved in a given time by water containing determinate amounts of various salts in solution. From my experiments on this subject, I have shewn that a comparatively small quantity of nitrates (especially ammonium nitrate) dissolved in a large quantity of pure water, causes that water to exercise a very solvent action upon lead. Pure distilled water dissolves about 2 mgm. of lead per litre, after twenty-four hours' exposure of the lead to the action of the water. Loch Katrine water about half as much lead as this. If a very

small quantity of a soluble carbonate be added to pure water, then the action of this water on lead is almost stopped. Similarly, if to a water containing nitrates there is added a small amount of a carbonate, the action which would, were nitrates alone present, be exercised on the lead is rendered very much less energetic.

Chlorides act, but not to so great a degree, as carbonates; and sulphates also act in a similar manner.

The results of my experiments I have embodied in the following table:—

Name of Salt in Solution.	Grms. per Litre.	Grains per Gall.	Lead Dissolved.					
			Grms. per Litre.			Grains per Gall.		
			24 hrs.	48 hrs.	72 hrs.	24 hrs.	48 hrs.	72 hrs.
Ammonium nitrate,	·02	1·4	13	...	25	·91	...	1·75
,, ,,	·04	2·8	15	15	32	1·05	1·05	2·24
,, ,,	·08	5·6	15	...	...	1·05	...	...
Potassium nitrate,	·02	1·4	2	2	...	·14	·14	...
Sodium sulphate,	·05	3·5						
Potassium nitrate,	·04	2·8	·8	1	1·2	·05	·07	·08
Sodium sulphate,	·212	14·7						
Potassium nitrate,	·045	3·1	...	...	·3	...	...	·021
,, carbonate,	·308	21·5						
Potassium nitrate,	·078	5·4	...	...	·5	...	...	·035
,, carbonate,	·504	35·2						
Calcium sulphate,	·252	17·5	·4	...	·8	·02	...	·05
,, ,,	·408	28·5	·4	...	1	·02	...	·07
Potassium carbonate,	·31	21·7	...	...	·2	...	...	·014
,, ,,	·516	36·1	...	...	·2	...	...	·014
Calcium chloride,	·25	17·5	·5	·5	·5	·04	·04	·04
,, ,,	·51	35·7	·3	...	·4	·02	...	·028
Sodium sulphate,	·20	14	...	...	·8	...	...	·05
,, ,,	·40	28	...	...	·5	...	...	·03
Ammonium nitrate,	·02	1·4	...	...	1·8	...	...	·126
Calcium chloride,	·06	4·2						
Ammonium nitrate,	·02	1·4						
Potassium carbonate,	·10	7	...	...	·4	...	...	·028
Sodium sulphate,	·20	14						
,, ,,	·20	14						
Potassium carbonate,	·04	2·8	...	...	·1	...	...	·007
Calcium chloride,	·01	7						
Loch Katrine,	...	...	1	1	1·5	·07	·07	·105
Distilled water,	...	...	2	2	3	·15	·15	·21

XII.—*The Jargons and Zircons of Ceylon.*

By M. H. COCHRANE, F.C.S.

Read before the Chemical Section, 15th April, 1872.

THE natural silicates of zirconium have of late attracted much attention among chemists, by reason of the researches of Messrs. Sorby and Forbes, and on account of the discovery of a supposed new earth associated with zirconia in these minerals. Many chemists have held the opinion that zirconia was in reality a mixed earth. Svänberg, in 1845, announced the discovery of a new element, to which he gave the name norium, in certain zircons and jargons of Ceylon and Siberia; but as he did not make known the results of any further inquiries, and as the researches of Berlin and other chemists yielded results entirely different from those of Svänberg, the existence of norium was rendered doubtful. Mr. Sorby, in 1869, announced the discovery, by means of the micro-spectroscope, of an earth having a totally different absorption spectra from that of any known element, and he succeeded in separating the new earth from zirconia. Mr. David Forbes, at Mr. Sorby's request, made a chemical analysis of a specimen of Ceylon jargon, and he also succeeded in separating the other earth from zirconia by taking advantage of the insolubility of zirconia in CONCENTRATED hydrochloric acid.

Having, through the kindness of a friend, obtained seven small specimens of jargon and zircon from different localities, I analyzed them by exactly the same process which Mr. Forbes adopted (a full description of which will be found in the *Chemical News*, vol. xix. p. 277). Except in one instance, everything was precipitated at the stage A. Mr. Forbes's analysis yielded the following numbers:—

Silica,	33·61	
Zirconia, A,	46·12	
Jargonia, B,	7·64	66·38
,, C,	12·62	
Ferric Oxide,	0·24	
	100·23	

My analyses gave the following results. The first specimen examined was an almost colourless Ceylon jargon:—

0·932 grammes gave—

Silica,	·316
Zirconia,	·604
Ferric Oxide,	no trace.

Its percentage composition was therefore—

Silica,	33·90 %
Zirconia,	64·80 ,,
	98·70

No. 2 was a hyacinth (a variety of jargon) of a very pale amber yellowish tint, perfectly transparent.

0·733 grammes gave—

Silica,	0·241
Zirconia,	0·471
Ferric Oxide,	0·015

Or when calculated to 100 parts—

Silica,	32·87
Zirconia,	64·25
Ferric Oxide,	2·04
	99·16

The third was a Norwegian zircon. It had a dark brownish yellow colour, and was almost opaque.

·505 grammes gave—

Silica,	·1710
Zirconia,	·3245
Ferric Oxide,	·0055

Or in 100 parts—

Silica,	33·86 %
Zirconia,	64·25 ,,
Ferric Oxide,	1·08 ,,
	99·19

The fourth was an almost colourless Ceylon jargon.

·5000 grammes gave—

Silica,	·1680
Zirconia,	·3200
Ferric Oxide,	·0045

When calculated to parts for 100, its composition was—

Silica,	33·60
Zirconia,	64·40
Ferric Oxide,	·90
	98·90

The next specimen examined was a zircon from Norway, and it presented the same physical appearance as the other Norwegian zircon.

·701 grammes gave—

Silica,	0·228
Zirconia,	0·449
Ferric Oxide,	·020

100 parts therefore contained—

Silica,	32·53
Zirconia,	64·05
Ferric Oxide,	2·85
	99·43

The sixth and seventh specimens examined were jargons from Ceylon. The sixth had a very slight tinge of colour. The seventh was perfectly colourless and transparent.

0·655 grammes of No. 6 gave—

Silica,	0·2165
Zirconia,	0·4370
Ferric Oxide,	traces.

Or in 100 parts—

Silica,	33·05
Zirconia,	66·71
	99·76

·4880 grammes of No 7 gave—

Silica,	0·1650
Zirconia, A,	0·2835
,, B,	0·0390

In 100 parts,

Silica,	33·81	
Zirconia, A,	58·29	66·32
,, B,	8·03	
	100·13	

It contained an almost inappreciable trace of iron. The precipitate marked zirconia B presented exactly the same look as the one marked A, and to all appearance the same chemical properties. When examined in the manner described by Messrs. Sorby and Forbes with the micro-spectroscope, it gave *no peculiar absorption bands;* from these results, therefore, I believe both substances to be identical.

XIII.—*On the Separation of Phosphoric Acid, Ferric Oxide, Alumina, Lime, and Magnesia.* By THOS. ROBERTSON OGILVIE, F.C.S.

Read before the Chemical Section, 29th April, 1872.

To separate and accurately estimate phosphoric acid, ferric oxide, alumina, and other bases, is one of the most difficult problems in analysis. It is also of great commercial importance, on account of the large quantities of ferruginous phosphatic minerals employed in the manufacture of superphosphate of lime; besides, owing to the increased demand for these minerals, they have considerably advanced in value; and merchants now demand from analysts a greater delicacy and minuteness of detail in their results than formerly. Another reason why a correct and speedy method of separation is of consequence has just been clearly shewn by a member of this section in the prominent part which ferric oxide and alumina play in the manufacture and composition of artificial manure.*

The method generally recommended is to dissolve the mineral in acid, evaporate to dryness, filter from the siliceous matter, then precipitate the lime as oxalate, which, after being washed, dried, and ignited, is weighed as carbonate. The filtrate is evaporated to a small bulk, and after the addition of citric acid to hold up ferric oxide and alumina, the phosphoric acid is separated as ammonio-magnesian-phosphate. To the fluid from this precipitate is added some nitrate of potash and sufficient carbonate of soda, to effect the decomposition of the chloride of ammonium, the whole evaporated to dryness, and ignited, so as to decompose and remove the citrate and oxalate of ammonia.

The residue is digested in acid for some time, and in the clear solution the ferric oxide and alumina precipitated with ammonia in the usual way. The magnesia, which is present in mineral phosphates in very small quantities, is estimated in another portion by

* "The part which Iron and Aluminium Oxides play in the Manufacture of Superphosphates, and on the Comparative Value of Mineral Phosphates," by T. L. Patterson, F.C.S.

separating the lime with oxalate of ammonia, the ferric oxide and alumina with acetate of ammonia, and allowing the filtrate, which contains a large quantity of phosphoric acid, to stand till the magnesia separates as ammonio-magnesian-phosphate.

This plan of separation is objectionable on two grounds; first, because phosphoric acid cannot be estimated with accuracy in presence of any considerable quantity of citrate and oxalate of ammonia; and, second, because the removal of the chloride of ammonia and the decomposition of the other ammoniacal salts are tedious and troublesome operations.

Another method of estimating the ferric oxide and alumina is given by Warrington, in his valuable paper on "The Analysis of Mineral Phosphates."* To the acid solution of the mineral large excess of caustic soda is added, which gives a precipitate of phosphate of lime and ferric oxide. The latter may be determined by the bichrome process, or separated from the phosphate of lime by dissolving in hydrochloric acid, and adding ammonia and large excess of acetic acid, when the ferric oxide may be filtered, washed, and ignited. The filtrate from the precipitate, got with caustic soda, contains the alumina, and is treated with chloride of barium to remove the phosphoric acid, with carbonate of soda to separate the excess of baryta, and with ammonia to precipitate the alumina. This mode of separation has not come into general use, owing to the great deal of manipulation involved and the difficulty of obtaining caustic soda free from alumina.

About a year ago Sonnenschein's method of separating phosphoric acid as phospho-molybdate of ammonia began to be adopted by our leading agricultural chemists, and since then the extraordinary discrepancies in their results have to some extent ceased. Regarding this method Warrington, in the paper already referred to, says, "it is inadmissible, from the large amount of phosphoric acid to be determined" in mineral phosphates; and Fresenius says, "that the phosphoric acid in the substance taken to operate upon should not be allowed to exceed 0·1 grm."†—which would be equivalent to 0·3 grm. of the mineral—a quantity so small that few chemists would care to work upon it in ordinary commercial analysis. Watts also repeats these statements; thus, "it cannot be depended upon for giving exact results, excepting when the quantity of phosphoric acid to be determined is very small."‡ By a number

* *Chemical News*, vol. x. p. 1.

† *Quantitative Analysis*, p. 274.

‡ Watts' *Dictionary of Chemistry*, vol. iv. p. 546.

of careful experiments, however, I have satisfied myself that perfectly satisfactory results may be got by using even 0·3 grm. of phosphoric acid. We may therefore separate the phosphoric acid in such minerals as apatite, coprolites, Rhenish phosphorite, and South Carolina phosphate, by this highly accurate method, and in the filtrate from the phospho-molybdate of ammonia estimate the bases.

Before referring to the latter point, which is the more immediate subject of this paper, I shall briefly give the details of Sonnenschein's process.

(*a*) *Separation of Phosphoric Acid.*—5 grms. of the mineral are dissolved in nitric acid, evaporated to dryness, re-dissolved in the same acid, filtered from the siliceous matter, and the solution made up to 250 c.c. The siliceous matter should be free from ferric oxide; if not, it must be digested with strong acid until white, and the filtrate added to the original solution. Of the fluid 50 c.c., containing 1 grm. of the mineral, are transferred to a beaker, a sufficient quantity of the molybdenum solution added, and the whole allowed to stand at a temperature of 40° C. until the precipitate thoroughly subsides, for which four hours are generally sufficient. The molybdenum solution is prepared in the following manner:—10 grms. of molybdate of ammonia are dissolved by gently heating in 40 c.c. of ammonia (·96 sp. gr.), and mixing it with 160 c.c. of a mixture of equal parts of nitric acid and water, care being taken to keep down the temperature. In this manner a solution of known strength is got, and little difficulty will be experienced in adding the requisite quantity—namely, 40 parts of molybdic acid for every 1 part of phosphoric acid present. The precipitate of phospho-molybdate of ammonia is filtered, washed with a mixture of equal parts of the molybdenum solution and water, dissolved in ammonia, and after neutralizing the greater part of the free alkali with hydrochloric acid, the phosphoric acid is precipitated by the addition of a slight excess of magnesia mixture as ammonio-magnesian-phosphate.

The filtrate from the phospho-molybdate of ammonia contains all the bases as nitrates; and Fresenius gives the following method of separating them from the molybdic acid:—Mix the acid fluid in a flask with ammonia to alkaline reaction, add sulphide of ammonium in sufficient excess, close the mouth of the flask, and digest the mixture. As soon as the solution appears of a reddish-yellow colour, filter off the fluid which contains sulphide of molybdenum and the alkaline earths, and wash the residue of sulphide of iron and alumina with water containing a little sulphide of ammonium, and determine the oxides by the usual methods. The sulphide of

molybdenum is next precipitated from the filtrate with hydrochloric acid, and the lime and magnesia estimated in the filtrate. This process, however, is too protracted and troublesome for ordinary use; and I propose to simplify it by taking advantage of the well-known property of the solubility of molybdic acid in free alkali and determine the bases in its presence.

(*b*) *Separation of Ferric Oxide and Alumina.*—The filtrate from the precipitate of phospho-molybdate of ammonia consists of a nitric acid solution of molybdic acid, ferric oxide, alumina, lime, and magnesia. It is placed in a beaker, allowed to cool, and then cautiously neutralized with ammonia, care being taken that the alkali is present only in slight excess. In a short time the ferric oxide and alumina will completely settle, when the supernatant fluid is filtered, the precipitate washed by decantation, transferred to a filter, and the washing finished. It is next re-dissolved in weak nitric acid, re-precipitated with ammonia, and carefully washed: the filtrate, if found to contain any lime, is added to the original solution. The precipitate is dried, ignited, and weighed as ferric oxide and alumina. By re-dissolving in hydrochloric acid the ferric oxide, after reduction with zinc, may be determined by the bichrome process, and the alumina found by difference.

(*c*) *Separation of Lime.*—The alkaline filtrate from the ferric oxide and alumina is treated with oxalate of ammonia, and allowed to stand in a warm place until the precipitate of oxalate of lime has completely subsided, which will take place in about three hours. The clear fluid is passed through a filter, the precipitate washed by decantation and otherwise, dried and ignited at such a temperature that no caustic lime is formed.

(*d*) *Separation of Magnesia.*—To estimate the magnesia in the solution from the oxalate of lime, the ammonia salts must be removed by evaporating it to dryness and igniting the residue, which will consist of magnesia and molybdic acid. It is digested with a little nitric acid to dissolve the magnesia, then considerable excess of ammonia added, to render the molybdic acid soluble; and in the clear solution the magnesia is precipitated with phosphate of soda as ammonio-magnesian-phosphate.

To test the correctness of this mode of separating phosphoric acid, ferric oxide, alumina, lime, and magnesia, a solution was prepared containing a mixture of these bodies in known proportions; and the following are the details of the experiments. In the one set (A 1 and B 1) magnesia was present, and in the other (A 2 and B 2) it was absent:—

	Used.	Found.		Used.	Found.	
	A.	A. 1.	A. 2.	B.	B. 1.	B. 2.
PO_5	·1984	·1982	·1989	·0397	·0409	·0409
Fe_2O_3	·0300	·0312	·0297	·1500	·1485	·1460
Al_2O_3	·0200	·0208	·0203	·1000	·1015	·0980
CaO	·3298	·3282	·3326	·1319	·1310	·1327
MgO	·0570	·0555	...	·1140	·1146	...
	·6352			·5356		
Found : Grms.		·6339	·5815		·5365	·4176
Used : Grms.		·6352	·5782		·5356	·4216

As nearly all mineral phosphates contain only a few tenths of a per cent. of magnesia, it would perhaps be preferable to take a fresh portion of the acid solution of the mineral, and separate the lime as oxalate, evaporate the filtrate to a small bulk, add ammonia, then acetic acid, and remove the ferric oxide and alumina as phosphates, render the fluid alkaline, and allow to stand for some time, when the small quantity of magnesia will be deposited as ammonio-magnesian-phosphate.

This plan of separating phosphoric acid, ferric oxide, alumina, and other bases, is simple and easy, as it involves only operations of the most elementary kind, and avoids the use of sulphuretted hydrogen. It is very expeditious; the bodies may all be separated in about twelve hours, and even this time may be shortened, if the separation of the phosphoric acid, ferric oxide, and alumina, in one portion of the acid solution, and the lime and magnesia in another, are gone on with simultaneously. Although the solution from the precipitate of phospho-molybdate of ammonia containing the bases is of considerable volume, it may be filtered very rapidly if the precipitates of ferric oxide and alumina and of oxalate of lime are allowed to settle properly. The method is also highly accurate; the phosphoric acid is determined by a process which may be placed in the front rank for delicacy and correctness, and the other bodies are got with all desirable precision.

XIV.—*On the Part which Ferric and Aluminic Oxides play in the Manufacture of Superphosphate, and on the Comparative Value of Mineral Phosphates.* By T. L. PATTERSON, F.C.S.

Read before the Chemical Section, 29th April, 1872.

As the consumption of phosphatic minerals in the production of artificial manures has increased to an enormous extent of late years, so it becomes a matter of the greatest importance that manufacturers should study carefully the influence of the several constituents of the raw material, on the production of the manufactured article. At present, importance is only attached to the per cent. of tri-calcic phosphate in a mineral phosphate, without regard to the other ingredients—in so much so, that most of them are purchased at a certain fixed price for each per cent. of this earth shewn in an analysis. Besides this, however, I will attempt to shew that it is equally important for manufacturers to know the per cent. of calcic carbonate, ferric and aluminic oxides, as well as how they are combined, to enable them to judge correctly what is the value of any mineral for the manufacture of superphosphate.

As I believe that a phosphatic mineral is only valuable for the superphosphate which can be made from it, I intend treating the subject from—what I may call—a decomposition point of view; and in the first part of this paper will discuss the reactions which take place when sulphuric acid is added to a mineral phosphate, containing ferric and aluminic oxides, and describe some experiments in support of these. In the second part I will shew, how the comparative value of these phosphates may be calculated.

The following analyses of three mineral phosphates illustrate very well all the points I wish to bring before the Section. Let us see what the reaction is when sulphuric acid is added to a phosphate such as No. 1, in quantity insufficient to render all the bases soluble. Decompositions represented by some or all of the following equations will be the result. To avoid complicity, I will not include the water of hydration.

	No. 1.	No. 2.	No. 3.
Tri-calcic Phosphate,	50·90	54·25	51·67
Di-magnesic Phosphate,	a little.	·55	·35
(= Tri-calcic Phosphate),	——	(·77)	(·49)
Calcic Carbonate, existing partly as Calcic Fluoride and Chloride,	9·64	24·47	14·55
Calcic Sulphate,	1·72	2·57	4·03
Ferric Oxide,	6·96	2·03	1·06
Ferrous Oxide,	——	·34	·42
Aluminic Oxide,	4·18	1·75	·89
Silica and Sand,	18·44	8·62	17·21
Loss on Ignition,	3·97	4·37	5·45
Loss @ 100° C.,	3·75	1·08	4·00
	99·56	100·03	99·63

(*a*) $Ca\,CO_3 + SO_3 = Ca\,SO_4 + CO_2$.
(*b*) $Ca_3\,(PO_4)_2 + 2SO_3 = Ca\,(PO_3)_2 + 2Ca\,SO_4$.
(*c*) $Fe_2O_3 + 3SO_3 = Fe_2\,O_3\,3SO_3$, and
(*d*) $Fe_2O_3 3SO_3 + Ca\,(PO_3)_2 = 2Fe\,PO_4 + Ca\,SO_4 + SO_3$, or,
(*e*) $Fe_2O_3 3SO_3 + Ca_3\,(PO_4)_2 = 2Fe\,PO_4 + 3Ca\,SO_4$, or,
(*f*) $3\,[Ca\,(PO_3)_2] + 2Fe_2O_3 = 4Fe\,PO_4 + Ca_3\,(PO_4)_2$, and,
(*g*) $4Fe\,SO_4 + O_2 + Ca\,(PO_3)_2 + Ca_3\,(PO_4)_2 = 4Fe\,PO_4 + 4Ca\,SO_4$.

Equations (*a*) and (*b*) are well established, and need no comment. Calcic fluoride will be decomposed in the same way as calcic carbonate, and is therefore represented by (*a*). I have not shewn the calcic fluoride separately in the above analyses, because for the purposes of the manufacturer it is unnecessary; and the small amount of silicon carried off by the liberated fluorhydric acid, is of no importance in the after calculation. The estimation of fluorine, too, is difficult and tedious, when present in small quantity; and, since it serves no useful purpose, it seems to me preferable to report the calcium existing as fluoride, as calcic carbonate—for so far as the calcium is concerned the ultimate result is the same.

When a mineral contains ferric and aluminic oxides—as they all do—these oxides will be attacked simultaneously with the tri-calcic phosphate, calcic carbonate, &c., and so we might expect to have ferric sulphate and aluminic sulphate—or still more acid salts—formed, according to equation (*c*), which in their turn react on a portion of the mono-calcic phosphate just become soluble, according to equation (*d*), setting free two molecules of sulphuric acid to attack a still untouched portion of tri-calcic phosphate. Or, again,

we may assume that the reaction represented in equation (*e*) takes place, where a molecule of ferric sulphate decomposes one of tri-calcic phosphate, with the production of two of ferric phosphate and three of calcic sulphate. Or, lastly, we may suppose, with Graham and Fresenius, that the acid attacks the calcic carbonate and phosphate in the first instance; then the soluble mono-calcic phosphate gradually reacts on the ferric and aluminic oxides, producing in the course of time the so-called "reduced phosphates," as represented by equation (*f*), where three molecules of mono-calcic phosphate are decomposed by two of ferric oxide, into four of ferric phosphate and one of tri-calcic phosphate.

Ferrous oxide, when present, simply replaces lime; but in the course of time, as it gradually absorbs oxygen, a portion of the mono-calcic phosphate will be precipitated, as shewn in equation (*g*).

None of these equations, however, appear to me to represent exactly the true decomposition of a mineral phosphate, on the addition of sulphuric acid. Yet, I think, two or more of them are sufficient to do so. The ferric and aluminic oxides exist, partly in a hydrated, and partly in an anhydrous condition, or at any rate one portion seems to be more hydrated than another. It is thus likely that equations (*c*) and (*d*), (*c*) and (*e*), or (*c*) (*d*) and (*e*) take place simultaneously with (*a*) and (*b*); the hydrated ferric and aluminic oxides taking part in the reaction, and afterwards, when the resulting superphosphate is kept for some time—as is the case in practice—the decomposition or reduction represented in equation (*f*) takes place, through the action of a portion of the oxide of iron and alumina, which remained unattacked in the process of manufacture. The following experiments were made in order, if possible, to throw some light on the decomposition of these minerals:—

Experiment 1.—A portion of No. 2 phosphate had a red colour after ignition, while superphosphate made from it, subjected to the same treatment, had a light-gray colour. This was also the case with No. 1 phosphate, and shews that, whatever be the reaction, the ferric oxide was not combined with phosphoric acid in the mineral, while it was in the superphosphate.

Experiment 2.—A sample of another phosphate which gave 10·52 per cent. of ferric and aluminic oxides, and 18·90 per cent. of sand, when digested with dilute chlorhydric acid, yielded *after ignition* only 5·79 per cent. of these oxides to the acid solution, and the residue weighed 23·26 per cent. When this residue was boiled in *nitric* acid, and the solution tested with molybdate of ammonia, it

was found free from phosphoric acid. The residue of the ignited portion was 4·36 per cent. in excess of that of the mineral before ignition, and 4·36 + 5·79 = 10·15, a result closely corresponding with the percentage of ferric and aluminic oxides found in the non-ignited mineral. Now, it is well known that hydrates of these oxides are less soluble after than before ignition; and this experiment shews that they were present in this mineral in that form.

As all these minerals lose weight on ignition over and above what they lose at 100° C., we may assume that the loss is principally water of hydration, and that it is in combination with oxide of iron and alumina. (Some of them contain organic matter, which is also driven off on ignition, but it usually exists in small quantity. It seems, however, to exert a reducing action on the ferric oxide, retaining a portion in the ferrous state. Nos. 3 and 2, which contain most organic matter, have also most ferrous oxide; and No. 1, having little more than a trace, contains none of that oxide.)

Experiment 3.—When a portion of any of these minerals, but especially No. 1, which contains most ferric and aluminic oxides, was digested with a solution of citrate of ammonia of 1·09 sp. gr. for about an hour on the top of the water-bath, and filtered, the filtrate had a very yellow colour, owing to the solution of a portion of ferric oxide. All the iron is not rendered soluble by this treatment, as the residue still remains coloured. This is doubtless due to the intimate admixture of the molecules of oxide of iron and alumina with the other ingredients of the mineral, so that it would be practically impossible to separate the whole of these oxides by this means. That a portion, however, is attacked and goes into solution, is another proof, I think, that they exist—partly, at least—in the hydrate form.

Experiment 4.—A mineral phosphate was dissolved with excess of dilute sulphuric acid, water added, and the whole thrown on a filter. To the filtrate which contained ferric and aluminic oxides, ammonia was added until a permanent precipitate was formed. The precipitate was filtered off, and the filtrate, which was now in the most neutral condition possible, was used to make further experiments.

Experiment 5.—To a portion of the filtrate from 4, pure precipitated phosphate of lime was added. The clear solution filtered from this mixture gave abundant evidence of iron, with ferro-cyanide of potassium; but after standing some days, the filtrate only shewed a minute trace of iron, when tested with potassic ferro- and sulphocyanides. The ferric sulphate in the original solution is thus shewn to have decomposed a portion of the tri-calcic phosphate with pro-

duction of ferric phosphate and sulphate of lime, as represented in equation (*e*).

Experiment 6.—To a second portion of the same solution was added a solution of alumina, ammonia alum; but even after standing some days it remained quite clear. Pure precipitated tri-calcic phosphate was now added. After the lapse of eight or ten days the mixture was filtered and tested with ammoina and acetic acid; an abundant precipitate of phosphate of alumnia was obtained. When the solution was boiled with phosphate of lime, filtered, and tested as before, a much less abundant precipitate was obtained. Thus, we see that although tri-calcic phosphate cannot remove aluminic oxide from a solution in the cold, yet it does so to a great extent when heated.* And as all superphosphate becomes highly heated in the process of manufacture, this re-action explains the reason why so little alumina is found in an aqueous extract. Its precipitation in this way is doubtless similar to that of ferric oxide, as represented in equation (*e*).

Experiment 7.—To a third portion of the filtrate from experiment 4 was added a solution of ferric ammonium alum, this being probably one of the most neutral soluble salts of that base. The mixture remained quite clear at first, but in the course of a minute a white floculent precipitate separated, which gradually increased on standing for some hours. Warrington has already shewn that the addition of an alumina salt to a solution of phosphoric acid gives no precipitate in the cold, while a precipitate goes down at once when the aluminic solution is replaced by a ferric one.† This difference between ferric and aluminic phosphate shews that the former salt requires a greater excess of acid to retain it in solution than the latter. On this account, I thought it would be interesting to determine the composition of the precipitate formed. Accordingly I prepared a small portion, filtered and washed it thoroughly in the cold, and dried it in the water bath.

·4248 grm. lost ·0820 grm. on ignition.
·4248 ,, gave ·1712 ,, ferric oxide, and
·3275 ,, gave ·2083 ,, pyro-magnesian phosphate.

The ferric oxide was estimated with $\frac{N}{10}$ chromate of potash, after

* I was unaware of the following statement in Storer's *Dictionary of Solubilities*, when the above experiments were made. At p. 483, he says, "Any insoluble c. phosphate of a protoxide is completely decomposed by any soluble salt of a sesquioxide, as an alum, for instance, either in the cold or at the boiling temperature, an insoluble salt of the sesquioxide being formed."

† *Chemical News*, vol. xxiii. p. 205.

reduction with zinc, and the phosphoric acid by the molybdate process. Calculated to p. c. the results are as follow, and correspond to the formula, $7Fe_2O_3 8P_2O_5$.

	Calculated from the Formula $7Fe_2O_3 8P_2O_5$.	Found.
Ferric Oxide, ...	40·21	40·30
Phosphoric Anhydride, ...	40·79	40·70
Loss on Ignition, ...		19·31
		100·31

When this acid phosphate of ferric oxide is digested with the ammonium citrate solution at 38° C., it goes easily into solution, as might be expected from the known solvent power of citric acid salts for those of ferric oxide. Whether this phosphate is formed in the manufacture of superphosphate remains to be proved. It seems very probable that a small quantity is produced simultaneously with ferric phosphate; but if it is, it is hardly possible for it to exist long as such, for the tri-calcic phosphate present will soon decompose it and bring it back to its normal state.

Experiment 8.—To a fourth portion of the solution from experiment 4 was added a solution of the double sulphate of ferrous oxide and ammonium. It remained perfectly clear for a couple of hours, when it began to be turbid, and after standing over night a considerable precipitate had separated. When this precipitate was filtered off, washed, and tested with potassium ferrid-cyanide, it was found perfectly free from ferrous oxide. The ferric phosphate which separated was doubtless formed, as represented in equation (*g*).

Experiment 9.—To a fifth portion of the same solution I added a quantity of well-washed hydrated ferric oxide. But little change was observed for a day or two. Gradually, however, the solution became opalescent—especially the supernatant fluid—when the oxide of iron collected at the bottom of the glass, and the red ferric oxide became of a lighter colour. These indications of decomposition became greater after the lapse of eight or ten days; and at the end of a month they were very evident, as the ferric oxide slowly and gradually precipitated the phosphoric acid, as represented in equation (*f*). A portion of the solution removed by filtration still gave abundant evidence of the presence of phosphoric acid.

Fresenius refers to another decomposition to account for these reduced phosphates. He supposes one molecule of mono-calcic phosphate to re-act on one of tri-calcic phosphate, with the production of two of the bi-calcic salt, thus—

$$Ca_3 (Po_4)_2 + Ca (H_2Po_4)_2 = 2 [Ca_2 (HPo_4)_2]$$

But no experiments being given in support of this hypothesis, and as the presence of ferric and aluminic oxides are sufficient to account for all the phenomena observed, I cannot regard this decomposition as at all probable.

Now, it follows from what I have said, that all samples of superphosphate which contain these oxides should also contain reduced phosphates. Chemists have tried to estimate them as though they were precipitated phosphate of lime. If a superphosphate be analyzed when it is made, and again at the end of twelve months or so, the phosphoric acid which has become insoluble during that period, is all that can properly be called "reduced." Yet the best process which has been devised for their estimation is capable of removing from a superphosphate more than that. I happen to have had beside me for some years three samples of superphosphate, which were analyzed at the time they were made. With the object, therefore, of testing the process, I have again subjected them to analysis; this time estimating the reduced phosphates by Fresenius, Neubauer, and Luck's indirect method, in which a neutral solution of ammonium citrate of 1·09 sp. gr. is used, as described in their paper "On the best Methods for the Analysis of Mineral Phosphates and Artificial Manures."* The following are the results with the phosphoric acid expressed as tri-calcic phosphate:—

	No. 1 S.		No. 2 S.		No. 3 S.	
	a	*b*	*a*	*b*	*a*	*b*
Soluble Phosphate, . . .	22·05	19·60	16·61	14·21	20·66	18·01
Insoluable Phosphate, . .	9·63	6·32	12·42	8·17	6·70	3·91
Reduced Phosphate, . . .	...	5·90	...	7·05	...	4·61
Total, . .	31·68	31·82	29·03	29·43	27·36	26·53

a is the composition of the superphosphate when made; *b*, that recently ascertained.

No. 1 S. is a sample of 190 tons made in October, 1868.
No. 2 S. ,, 70 ,, December, 1868.
No. 3 S. ,, 75 ,, January, 1869.

These samples have thus lain in my possession for upwards of three years, during which time they have gone back in soluble phosphate to the extent of—

* Fresenius, Zeitsch, vol. x. part 2.

2·45 per cent., in No. 1 S. sample.
2·40 ,, No. 2 S. ,, and
2·65 ,, No. 3 S. ,,

Thus, we see that the so-called "reduced phosphates" are very much greater, when estimated by this indirect method, than is represented by the difference of the soluble phosphate in the superphosphate at the time they were made, and that found in them after the lapse of three years. The citrate of ammonium filtrate comes away highly coloured with iron, because it has dissolved ferric phosphate —it has also removed aluminic phosphate and *all* the calcic sulphate. The insoluble residue still contains phosphoric acid, ferric and aluminic oxides; and as in these samples it contained no lime, the phosphoric acid must have been in combination with the ferric and aluminic oxides. If, then, as I have just shewn, the citrate solution removes more phosphoric acid than is represented by the actual going back of the superphosphate in three years, the indirect method—giving a much higher per cent. for these phosphates than it ought—is quite unsuitable for their estimation. If, on the other hand, we are to understand as reduced phosphates all phosphoric acid, which has combined with ferric and aluminic oxides in the process of manufacture—there being no phosphoric acid in combination with these oxides in the raw material, while it is all combined in superphosphate—and if, moreover, it be admitted that these reduced phosphates are chiefly phosphates of iron and alumina, and not phosphate of lime,—it is again apparent that the citrate solution cannot be used to separate them, as it failed to dissolve all the ferric and aluminic phosphate in the insoluble residue, from the three samples above quoted.

The reduction of these superphosphates may be explained by some of the equations given above. The water solution of an artificial manure seldom contains more than a trace of ferric and aluminic oxides, even when newly made, so that the 2·45 per cent. gone back in No. 1 S., for example, could not have been the result of a decomposition such as (*d*), but it is highly probable it has been formed in the way represented by equation (*f*). Any ferrous oxide which a mineral contained would of course also reduce a portion of the phosphoric acid, as represented in (*g*); but in one case only—viz. (*f*)—does one-third of the phosphoric acid reduced exist as tricalcic phosphate, the remainder being ferric phosphate and aluminic phosphate. The residue of the reduced phosphate in No. 1 S.—5·90 − 2·45 = 2·45—was in all probability never soluble at all, it

having been produced in the process of manufacture, according to equations (*c*), (*d*), and (*e*) together, or (*c*) and (*e*) alone.

Summarizing what I have said, we see in the first place that when a mineral phosphate is mixed with sulphuric acid, the ferric and aluminic oxides, which were originally present in the form of hydrate, become partially converted during the reaction, and completely after the lapse of an indefinite period, into the insoluble phosphates of these oxides; and, lastly, that the so called "reduced phosphates" consist principally of a portion of the phosphates so formed.

This brings me to the second part of my paper, viz., The comparative value of mineral phosphates. Continuing to speak of these from a decomposition point of view, and assuming that the tri-calcic phosphate is the only valuable ingredient they contain, it will be necessary to consider how far their value is modified by the presence of ferric and aluminic oxides, which, I have just shewn, reduce or render insoluble a portion of the phosphoric acid.

It is well known that superphosphate of lime, when applied to the soil, soon has its phosphoric acid precipitated, as insoluble phosphate of lime, by means of the earthy carbonates contained therein. And it is admitted by all chemists that such precipitated phosphate possesses with soluble phosphate of lime the same value as a fertilizer. It is also admitted by chemists that the great value of soluble or precipitated phosphate of lime, compared with mineral phosphates, depends wholly on the much greater solubility of the former. By determining, then, the solubility of the ferric and aluminic phosphates, we will be in a position to form more correct notions as to their manurial value. I therefore made two other experiments to clear up this point.

Experiment 10.—Carbonic acid gas was continuously passed through distilled water holding pure precipitated tri-calcic phosphate (thoroughly washed and dried in the water bath) in suspension for five hours, when the undissolved portion was removed by filtration. On heating a portion of the filtrate to boiling with dilute nitric acid and ammonium molybdate, a considerable yellow precipitate was obtained. To another portion of the filtrate was added one drop of a solution of ferric alum, when a slight precipitate was immediately produced. After standing some time, it collected at the bottom of the vessel. The tri-calcic phosphate solution was now added in excess, and the ferric phosphate filtered off. The filtrate gave no blue colour with ferrocyanide, and only a faint reddish tinge with sulphocyanide of potassium when acidified with chlorhydric acid.

To a third portion of the filtrate ferric alum was added in excess, when the precipitate at first formed re-dissolved.

To a fourth portion of the filtrate a drop of aluminic alum was added. An opalescence was immediately produced, and after standing some time a precipitate of aluminic phosphate separated. When the aluminia solution was added in excess, the precipitate was re-dissolved. On separating the aluminic phosphate precipitate—after a carefully conducted precipitation, to insure the merest trace of alum in excess—a drop of the ferric alum produced a second precipitate of ferric phosphate. Thus shewing aluminic phosphate to be the more soluble of the two.

Experiment 11.—Very finely ground and washed coprolite, suspended in distilled water, was subjected to a continuous stream of carbonic acid gas during five hours, as was the tri-calcic phosphate in experiment 10. The undissolved portion was then filtered off, and the filtrate, when tested with molybdenic solution, only gave a slight trace of a yellow precipitate. On evaporating the filtrate to one-fourth, however, good evidence of the presence of phosphoric acid was obtained. Aluminic alum produced in the filtrate a barely perceptible opalescence, which, after standing some time, collected as a precipitate at the bottom of the vessel. This precipitate was removed by filtration and tested with molybdenic solution after treatment with dilute nitric acid. A considerable yellow precipitate proved the presence of phosphoric acid. The filtrate evaporated to one-fourth only gave a trace of a yellow precipitate with molybdic acid solution. To another portion of the coprolite solution I added a drop of ferric alum;—an opalescence was immediately produced. With a slight excess of the precipitant, the precipitate has a yellow colour, caused by the calcic carbonate in solution throwing down hydrated ferric oxide; when the precipitant is in great excess, no precipitate at all is formed, or that at first formed is re-dissolved. (This is also the case with an excess of the aluminic solution.) On removing the yellow precipitate thus formed, and dissolving it in dilute nitric acid, abundant evidence of the presence of phosphoric acid was obtained on heating the solution with molybdic acid to boiling. The filtrate when evaporated to one-fourth, gave the merest trace of a yellow precipitate when tested with molybdenic solution. The results of these experiments are similar to some made by Warrington,* with another object in view.

I thought this precipitation method of experimenting preferable to dissolving separate portions of each in carbonic water, and

* *Chem. News*, vol. xvi. p. 253, and vol. xxi. p. 221.

determining the amount dissolved; as it produces a minute quantity of these phosphates in the very best possible condition for solution, and—as it has turned out—shews at a glance how insoluble they are, compared with ground coprolite and pure phosphate of lime. If we arrange them in the order of their solubilities, coprolite will come first, aluminic phosphate next, and ferric phosphate last. I forget the actual solubility of coprolite, and cannot remember where I have seen it stated. Bischof* gives the solubility of wavellite as 1 part in 6,828,000 parts of carbonic water; but it is likely the precipitated aluminic phosphate here referred to is much more soluble than that. And ferric phosphate must be very slightly soluble indeed, when we consider the extreme delicacy of the sulphocyanide test for iron, and the molybdenic test for phosphoric acid, and that only minute traces of these bodies were found in the filtrate from the ferric phosphate precipitate.

Now, most agricultural authorities are agreed that ground coprolite in a superphosphate is worthless. Voelcker says,† "Insoluble phosphates, in the shape of coprolite powder, are not worth anything in an artificial manure, for they are too insoluble to be taken up by the turnip crop." Of what value, then, are ferric and aluminic phosphates in a manure? These experiments shew that they have none whatever. But it may be said the manufacturer can hardly be expected to lose the phosphoric acid which has thus become unavailable; neither, would I say, has he any right to charge it to the farmer as precipitated phosphate of lime. Rather let him take care not to pay more for the raw material which contains these objectionable oxides than it is worth; then he will be able to sell his superphosphate as it ought to be sold—namely, as valuable only for that portion of phosphoric acid soluble in water.

At first sight it might appear that Warrington's experiments on the absorptive power of soils‡ contradict all that I have said regarding the worthlessness of the phosphates of peroxide of iron and alumina. He has tried to prove that hydrated ferric and aluminic oxides in the soil ultimately convert all phosphoric acid added as manure into basic phosphates of these oxides. But a careful study of his results, shews I think, that the conversion of precipitated phosphate of lime into ferric and aluminic phosphates is very gradual, and points to the probability of its being a long time before all the phosphoric acid could be thus transformed. My

* *Chem. and Phys. Geology*, vol. ii. p. 34.

† Art. "Manure" in Ure's *Dict.*, vol. iii. p. 50.

‡ *Chem. News*, vol. xxi. p. 207.

own experiments are in the same direction. In experiment 9 I have shewn how very slowly hydrated ferric oxide removes phosphoric acid from an acid solution. And again, in super-phosphate, how slowly ferric and aluminic hydrates precipitate a portion of the phosphoric acid. The fact still remains, too, that root crops are greatly increased in quantity by the addition of super-phosphate of lime to the soil; from the precipitated phosphate of lime thus produced, being comparatively more soluble, and easily assimilated by plants. These considerations lead me to think, that the rootlets of plants abstract tri-calcic phosphate from its carbonic acid solution, almost as quickly as it is dissolved, and before the ferric and aluminic oxides have had time to absorb any appreciable quantity of it. It is only after the roots have taken what they require that the absorptive action of these oxides will come into full play, and go on precipitating the remaining portion, to give it up slowly again at some future period, to a crop requiring little phosphoric acid; for, as the rootlets feed almost wholly on salts in solution, it is difficult to conceive how a quick growing plant, which requires much phosphoric acid, could assimilate a sufficient quantity in a short time from such insoluble materials, and ones, too, the affinities of whose components are so very great. And as, moreover, mineral phosphates are only treated with acid to make them more soluble, for the purpose of rendering the soil more productive and increasing our crops, we are driven to the inevitable conclusion, that any phosphate in a manure less soluble than precipitated tri-calcic phosphate,—and certainly any less soluble than ground coprolite—must be worthless as a fertilizer.

Such being the case, I submit that the present method of valuing a mineral from the percentage of tri-calcic phosphate alone which it contains, is a very erroneous one, and ought to undergo some modification, whereby these oxides are taken into account in the valuation. To do this, it will be necessary to calculate, *first*, what we may call the "net realizable per cent." of tri-calcic phosphate, obtainable from any sample; and, *second*, the weight per cent. of sulphuric acid required to reduce it to this condition. With these data, and the known composition and value of some sample taken as a standard, it will be possible to ascertain very closely indeed the comparative value of another mineral phosphate.

First, then, in order to find the net realizable per cent. of tri-calcic phosphate, we must know, by analysis, the actual per cent. of tri-calcic phosphate; ferric, aluminic, and ferrous oxides which the mineral contains. Knowing this, it is simply necessary to calculate

for each of these oxides, equivalent quantities of tri-calcic phosphate; and, deducting the sum of the three from the actual per cent. of phosphate of lime, the remainder will be the net realizable tri-calcic phosphate in the sample. If the ferric oxide be multiplied by the factor 1·9375 $\left(\frac{155}{80} = 1{\cdot}9375\right)$, the aluminic oxide by 3·0156 $\left(\frac{155}{51{\cdot}4} = 3{\cdot}0156\right)$ and the ferrous oxide by 2·1528 $\left(\frac{155}{72} = 2{\cdot}1528\right)$, the amounts of tri-calcic phosphate equivalent to these oxides are at once found.

And, second, to calculate the weight of sulphuric acid required to reduce the mineral to the condition of net phosphate of lime, we must know the percentage of calcic carbonate, besides that of the iron and aluminium oxides. If any ferric and aluminic oxides exist as phosphates of these oxides—and this is sometimes the case—such oxides are not taken into account in this calculation. The acid I propose to use has a gravity of 1·734 = 147° T., and contains 80 per cent. of H_2SO_4. This is of commercial strength, so that a value can always be assigned to it.

1	Molecule of	$CaCO_3$	requires	1 of	H_2SO_4
1	,,	Fe_2O_3	,,	3	,,
1	,,	Al_2O_3	,,	3	,,
2	,,	FeO	,,	3	,,

Calculating a factor for each of these, whereby the weight of acid of the above strength may be found for all of them, we have—

For %	$CaCO_3 \times 1{\cdot}225$	= acid @ 147° T.
,,	$Fe_2O_3 \times 2{\cdot}297$	= ,,
,,	$Al_2O_3 \times 3{\cdot}575$	= ,,
,,	$FeO \times 2{\cdot}552$	= ,,

and the sum of the products is the acid required.

It is thus assumed, that were that quantity of acid added to a mineral phosphate, and the mixture allowed to stand some time, as is the case in practice, the product would consist—so far as the bases and acids were concerned—of tri-calcic phosphate, ferric phosphate, aluminic phosphate, and calcic sulphate; and the amount of the first in the mixture, would be what I have called the net realizable phosphate of lime.

Before concluding, I will just go through the calculation necessary to find the value of one of these phosphates. In No. 2 sample we have—

Fe_2O_3	= 2·03 %	× 1·9375	= 3·93	$Ca_3(PO_4)_2$.
Al_2O_3	= 1·75 %	× 3·0156	= 5·28	,,
FeO	= ·34 %	× 2·1528	= ·73	,,
		Total,	9·94	

and 55·02 – 9·94 = 45·08 per cent. of net tri-calcic phosphate. The acid is found as follows :—

$CaCO_3$	= 24·47 %	× 1·225	= 29·97	Acid.
Fe_2O_3	= 2·03 %	× 2·297	= 4·66	,,
Al_2O_3	= 1·75 %	× 3·575	= 6·26	,,
FeO	= ·34 %	× 2·552	= ·87	,,
			41·76	

100 parts of this phosphate are thus shewn to yield 45·08 parts of net phosphate of lime, with the consumption of 41·76 parts of sulphuric acid, of 1·734 sp. gr.

When Nos. 1 and 3 are calculated in the same way, we see at a glance that No. 3 is superior to No. 2, although it contains less phosphate of lime; and again, because No. 1 contains so much ferric and aluminic oxides, how inferior it is to either of them:—

	Net Tri-calcic Phosphate.	Sulphuric Acid. Sp. gr. = 1·734.
No. 1, . . .	24·82, with a consumption of	42·69;
No. 2, . . .	45·08 ,,	,, 41·76; and
No. 3, . . .	46·52 ,,	,, 24·70.

Taking now the price of vitriol @ 147° T., at 80s. per ton, and that of the mineral phosphate, No. 3, as a standard, at 52s. per ton, which prices are about their present market value, we can easily calculate the comparative value of the other two. 52s. for 46·52 p. c. = 1·118s. per one per cent. In No. 2 we have 45·08 × 1·118s. = 50·4s. per ton. But as No. 2 consumes more acid than No. 3, the value of the excess must be deducted from the price so found, to place the two on equality. 41·76 – 24·70 = 17·06 acid @147° T.@ 80s. per ton = $\frac{17·06}{100}$ × 80s. = 13·65s. And 50·4s. – 13·65s. = 36·75s., or 36s. 9d., as the true value of No. 2 compared with No. 3 @ 52s. per ton. When No. 1 is calculated in the same way, it is found to have a value of only 13·35s., or 13s. 4d. per ton!

MINUTES.

November 1, 1871.

THE Session of the Philosophical Society of Glasgow was opened this evening, Dr. Bryce, the President, in the Chair.

The President delivered the Annual Address; for which, on the motion of Professor Grant, he received the thanks of the Society.

Mr. Archibald Arrol and Mr. George Watson were appointed Auditors of the Treasurer's Accounts.

November 15, 1871.

The Philosophical Society of Glasgow met this evening for the Seventieth Annual Election of Office-bearers—Dr. Bryce in the Chair.

The following were elected members of the Society, viz.:—

Dr. James Roberton, Professor of Conveyancing in the University of Glasgow; Professor Gustav Bischof, Anderson's University; Mr. Arnulf Schertel, Chemical Assistant, Anderson's University; Mr. Peter W. Dixon, Merchant, 19 Elmbank Crescent; Mr. William G. Lindsay, Accountant, 3 West Regent Street; Mr. Michael Cochran, Overdale, Langside.

The Secretary read the Annual Report by the Council on the State of the Society, which was approved of.

REPORT BY THE COUNCIL ON THE STATE OF THE SOCIETY.

1. *State of the Membership.*—The number of members on the roll of the Society at the beginning of the session of 1870–1871 was 483. The number of new members admitted during the session was 47; and one name was re-instated from the suspense-list, making a total membership of 531. From this number there fall to be deducted, for arrears of annual subscription, 1; resigned, 10; left

Glasgow and placed on suspense-list, 6; dead, 4; total, 21; leaving on the roll at the beginning of the session of 1871–72, 510 members. Of this number only 5 are in arrears of subscription for one year, and one for two years; the number of members on the roll being the largest, and the arrears of payments, so far as is known, being the least, in any year of the Society's existence.

2. *The Proceedings.*—The printed *Proceedings* of the Society in the Session 1870–71 occupied 147 pages, forming the third and concluding part of the Seventh Volume.

The opening address by Dr. Bryce, the President, comprised, along with obituary notices of deceased members of the Society, a review of the recent progress of science. At the subsequent meeting, after the annual election of office-bearers, Mr. James R. Napier exhibited and explained Stevenson's "Light for Ships," a brief notice of which appears in the *Proceedings*. This is followed by a paper entitled, "The Farmer and the Chemist," by Mr. James Napier, senior. Mr. James R. Napier next described a Wind-Direction Rain-Gauge of his own contrivance. At the only meeting held in December, Professor Sir William Thomson explained and illustrated the Attractions and Repulsions due to Vibration, observed by Guthrie and Schellback. When the Society re-assembled in January, Dr. Bryce read a paper on "The Age of the Rocks of the Central Highlands," the publication of which was postponed. Mr. Alexander S. Herschel afterwards gave an account of the "Observations in Spain of the Recent Eclipse of the Sun;" and was followed on the same evening by Dr. T. E. Thorpe, who described "The Eclipse Observations in Sicily." The discussion on these two communications is printed in the *Proceedings*, along with the papers themselves. At a subsequent meeting Mr. Alexander S. Herschel described and illustrated Helmholtz's Analysis of the Vowel Sounds. Mr. James R. Macfadyen contributed an elaborate paper on "The Theory of the Death Rate, with Measurements of the Comparative Force of Mortality in Glasgow and other Cities." The reading of this communication gave rise to a lively discussion, the report of which accompanies the printed paper. The other papers read before the Society, and embraced in the *Proceedings*, were the following, viz.:—"Note upon Scientific Education in National Elementary Schools," by the Rev. Henry W. Crosskey, with reported discussion. "On the Chemical Constitution of Paraffin," by Dr. T. E. Thorpe. "Notes on the Cemetery of Staglieno, near Genoa," by Mr. Charles Heath Wilson, communicated by Dr. Gairdner, and printed in abstract, with report of dis-

cussion. "On some Evidences as to the very early Use of Iron, and on certain old bits of Iron in particular," by Mr. St. John Vincent Day. The *Proceedings* were, as usual, indebted to the Chemical Section for several important contributions, viz. :—"On some Sources of Error in Volumetric Analysis," by Mr. Robert Tatlock. "Notes of Analyses of an Earthball and Intestinal Calculi from the Horse," by Mr. James F. Stark. "Preliminary Notice on the Action of Chloride of Zinc upon Morphia and Papaverine, and on the Action of Nitrous Acid, Bleaching Powder, and Hydrochloric Acid upon Morphia, and some notice of a new Opium Base," by Mr. E. L. Mayer.

The following papers were read before the Society, but not included in the printed *Proceedings*, viz. :—"On the Relationship of the Post-tertiary Beds of England to those of Scotland," by the Rev. Henry W. Crosskey. "On the Discovery of Fragments and Boulders of Granite in Schistose Rocks in Islay," by Mr. James Thomson. "Experimental Illustrations of the Dynamical Theory of Ships' Rolling," by Sir William Thomson, who on the same occasion exhibited and described "New Forms of Permanent Daniell's Batteries."

The *Proceedings* contain the annual report from the Chemical Section, given in by Mr. Robert R. Tatlock, the Secretary; and the annual report from the Sanitary and Social Economy Section, given in by Mr. David G. Hoey, the Secretary. These reports bore testimony to the session having been one of much activity on the part of both Sections. It is proper here to remind the Society that the report of the Sanitary and Social Economy Section concluded by expressing the opinion, that greater interest might be excited by the existence and proceedings of the Section being kept more prominently before the members of the Parent Society; "and to this end the Council of the Section respectfully ask that the President of the Society should, at the close of each fortnightly meeting of said Society, intimate the next meeting of this Section and the paper to be then brought up."

In the course of the past year the Council of the Society has been greatly aided in the administration of the Society's funds by a Finance Committee, which has met at stated times for the purpose of examining and passing accounts, and regulating the outlay of money.

The Abstract of Treasurer's Account having been printed in the circular calling the present meeting, was held as read, and approved of. It is in substance as follows:—

Abstract of Treasurer's Account. Session 1870-71.

Dr.

1870.—*Nov.* 1.					
To Balance from last Session—					
In Union Bank of Scotland,				£1	12 5
1871.—*Oct.* 31.					
To Entry Money and Dues from 47 New Members, at 42*s.*,				98	14 0
,, Annual Dues from 5 Original Members, at 5*s.*,	£1	5	0		
,, Annual Dues from 452 Members, at 21*s.*,	474	12	0		
,, Annual Dues from 17 Members, for two years,	35	14	0		
,, Annual Dues from 2 Members, for three years,	6	6	0		
				517	17 0
,, Call of 5*s.* towards Expense of Re-furnishing,—280 Members, at 5*s.*,				70	0 0
,, Chemical Section.—4 Associates for 1869-70, and 36 Associates for 1870-71, at 5*s.*,	£10	0	0		
Less Expenses,	1	0	10		
				8	19 2
,, Sanitary Section.—7 Associates for 1870-71, at 5*s.*,	£1	15	0		
Less Expenses,	0	3	4		
				1	11 8
,, Corporation of Glasgow, interest on "Exhibition Fund,"	£49	3	4		
,, Interest from Bank,	1	10	7		
				50	13 11
,, *Proceedings* sold,	£1	8	9½		
,, Catalogues sold, 66 at 1*s.*,	3	6	0		
				4	14 9½
				£754	2 11½

Cr.

1870.—*Nov.* 1.					
By Balance from last Session—					
Due Treasurer,				£41	1 5½
1871.—*Oct.* 31.					
By Salaries and Wages,				143	7 9
,, New Books and Binding,	£126	0	6		
,, Printing Catalogue, *Proceedings*, Circulars, &c.,	140	18	8		
,, Postage and Delivery of Circulars, &c.,	24	12	8½		
,, Diplomas and Stationery,	20	18	0		
,, Lithographing Plates, Circulars, &c.,	22	9	9		
				334	19 7½
Carry forward,				£519	8 10

	Brought forward, . . .				£519	8 10
By Rents, ...	£130	0	0			
,, Fire Insurance, Gas, Coal, Cleaning, &c., .	20	0	0			
,, Petty Charges and Sundries, ...	3	8	3			
				153	8	3
,, Subscriptions to Societies:—						
Ray Society, 1871, ...	£1	1	0			
Palæontographical Society, 1870 and 1871, .	2	2	0			
				3	3	0
,, Architectural Section—Expenses for year 1870, . . .				10	0	7
,, Furnishing of Rooms, &c., ...				51	19	11
,, Balances—In Union Bank, ...	£2	3	2			
In Treasurer's hands, . . .	13	19	2½			
				16	2	4½
				£754	2	11½

GLASGOW, *8th November*, 1871.—We, the Auditors appointed at a Meeting of the Society, held on 1st instant, have examined the Treasurer's Accounts, of which the above is a [condensed] Abstract, and found them correct, the Balance in Union Bank at 31st October last being Two pounds three shillings and two pence; and in Treasurer's hands, Thirteen pounds nineteen shillings and two pence half-penny sterling.

(Signed) ARCHD. ARROL.
GEORGE WATSON.

The Librarian read a Report on the State of the Library, which was approved of.

LIBRARIAN'S REPORT.

During the year which has elapsed since the last report of your Library Committee, such improvements in the working of the Library as were then in course of being carried out have been completed. The Committee believe that those members who are in the habit of frequently consulting the Library have experienced much facility in obtaining such books as they require to use, this being a direct result of the system of press-marks introduced in connection with the new Catalogue which has been published and circulated since the date of the last report.

In the purchase of books your Library Committee have been actuated as much as possible with the same endeavour which was expressed in their last report, namely, to provide such works in science as appear to be strictly characteristic of a "Philosophical Library;" and in this they have—whilst obtaining the more important recent publications in various European languages—laboured, as far as possible, to fill up gaps which they found to

exist in several of the departments of literature which the Library embraces. During the past year the number of volumes purchased has been 160, besides the periodical literature. Thirty-nine periodical publications are regularly supplied to the Library,—so that that portion of the Society's funds expended in the purchase of books has added upwards of 200 volumes to the Library in the past twelve months. A list of the purchases is appended to this report, including a record of the periodical literature received at the Library. (See list at close of present Part.)

In the matter of donations no less than 122 volumes and pamphlets have been received; and the Librarian has transmitted the thanks of the Society to the several donors in the usual form. A list of the donations will be found appended to this report. (See list at close of present Part.)

The system of exchange of publications with learned Societies and individuals has been extended. The Society has on its exchange-roll seventy different learned Societies and individuals in various parts of the world, to whom the *Proceedings* are sent, and from whom a great mass of literature is received in return. Your Committee feel it an especial duty to mention the receipt of a donation of nineteen volumes of reports and essays on several scientific subjects, from the Smithsonian Institution of Washington, U.S.A.

In the matter of accommodation the Library Committee regret to have to report it as entirely insufficient for the growing requirements of the Library. Already some confusion has resulted from the necessity of having to locate some of the books in situations which are not intended for their reception, and it is evident that further accommodation will shortly be imperative. Indeed, your Library Committee cannot too strongly urge the necessity of increased accommodation for books being speedily provided.

As regards the Library of the Architectural Section, it has been placed in a separate room, and book-cases enlarged and improved at the expense of the Philosophical Society for its reception; so that the fears which were entertained for its safety at the date of the last report no longer exist. The Library of the Architectural Section has been increased by fifteen volumes, a list of which is appended to this report.

The Society then proceeded to the election of Office-bearers.

The following Office-bearers retired by rotation, viz.:—

Dr. James Bryce, from the office of President; Dr. Allen

Thomson, from the office of Vice-President; Mr. William M'Adam, Mr. Edward C. C. Stanford, and Mr. Sigismund Schuman from the Council. A fourth vacancy was caused by the death of Mr. Moses Provan, who would, however, in ordinary course, have been one of the four Members of Council retiring this year.

Dr. Bryce proposed that Dr. Robert Grant, Professor of Astronomy in the University of Glasgow, be elected to the office of President, which was agreed to by acclamation.

Dr. Bryce proposed that Dr. John Young, Professor of Natural History in the University of Glasgow, be elected to the vacant office of Vice-President, which was agreed to by acclamation.

On the motion of Dr. Bryce, the re-election of Mr. St. John Vincent Day as Librarian, of Mr. John Mann as Treasurer, and of Mr. William Keddie as Secretary, was agreed to by acclamation.

For the four other vacancies in the Council several gentlemen were proposed, and voted upon by ballot.

Mr. John Campbell and Mr. Macarthur were appointed Scrutineers of votes.

The Scrutineers reported that the following gentlemen had been elected, viz.:—

Dr. Bryce, Mr. David Rowan, Dr. Andrew Fergus, Dr. James Morton.

After the election, Mr. H. L. Seligmann exhibited a new Calculating Machine, and explained its construction.

Mr. James Thomson, read "Notes on the Tailless Trout of Islay."

Dr. Bryce communicated some notices of recent Geographical Discovery.

Dr. Young proposed that the Philosophical Society express its approbation of the suggestion that the Chair of Natural History in the University of Glasgow should be separated from the Chair of Geology, and respectfully urge upon the authorities of the University to take such action as may be deemed proper in order to secure this adjustment.

The proposal, on being seconded by Dr. Bryce, was remitted to the Council for consideration.

Dr. Wallace moved that the thanks of the Society be given to Dr. Bryce for the able manner in which he had discharged the duties of President during the last three sessions, and for his distinguished services to the Society.

The motion was seconded by Mr. William R. W. Smith, and cordially agreed to.

OFFICE-BEARERS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW,

1871-72.

President.

PROFESSOR ROBERT GRANT, M.A., LL.D., F.R.S.

Vice-Presidents.

PROFESSOR SIR WILLIAM THOMSON, LL.D., F.R.S.
PROFESSOR JOHN YOUNG, M.D., F.R.S.E.

Librarian.

MR. ST. JOHN VINCENT DAY, C.E., F.R.S.E.

Treasurer.

MR. JOHN MANN, C.A.

Secretary.

MR. WILLIAM KEDDIE, F.R.S.E.

Other Members of Council.

DR. J. G. FLEMING.	MR. HORATIO K. BROMHEAD, I.A.
MR. JAMES NAPIER, F.C.S.	DR. W. WALLACE, F.R.S.E., F.C.S.
MR. DANIEL MACNEE, R.S.A.	DR. JAMES MORTON.
MR. ROBERT GRAY.	DR. ANDREW FERGUS.
MR. WILLIAM R. W. SMITH.	MR. DAVID ROWAN.
MR. JAMES ANDERSON.	DR. JAMES BRYCE.

November 29, 1871.—PROFESSOR GRANT, LL.D., *the* PRESIDENT, *in the Chair.*

On the minute of the annual election of Office-bearers at last meeting having been read, Dr. Grant expressed his sense of the honour which the Society had conferred upon him by appointing him to the office of President.

The following were elected members of the Society, viz.:—

Dr. J. St. Clair Gray, M.B., C.M., &c., 15 Newton Terrace; Mr.

John Chalmers, 251 Renfrew Street; Mr. Gavin Irving Dickson, Accountant, 37 West George Street; Mr. John Hildesheim, 21 St. James Street; Mr. George Munsie, 345 Bath Street; Mr. Thomas Martin, 19 Rupert Street, Great Western Road; Mr. Robert Macnab, 135 Buchanan Street; Mr. Dugald Bell, Merchant, 27 Lansdowne Crescent.

Dr. Bryce moved that the Society should concur with any other Societies or Public Bodies in the City, who might agree to invite the Social Science Association to hold an early meeting in Glasgow.

Mr. William R. W. Smith having seconded the motion, it was submitted to the Society and unanimously approved of.

The President delivered the first part of an address "On the Astronomy of the Nineteenth Century;" the second part being reserved for next meeting.

December 13, 1871.—*The* PRESIDENT *in the Chair.*

The following were elected members of the Society, viz.:—

Mr. Paul Rottenburg, Merchant, 16 Windsor Terrace, St. George's Road; Mr. Angus Mitchell, 42 Miller Street; Mr. Richard M'Culloch, Writer, 93 West Regent Street; Mr. Francis Napier, Engineer, 100 Hydepark Street; Mr. John King, 13 Renfield Street; Mr. John Bruce Murray, 7 Cochran Street; Mr. George Murray, 7 Cochran Street; Mr. Thomas Davidson, jun., 33 and 41 Garngad Hill; Mr. William Paris, jun., Glasgow Iron Works; Mr. Neil Sinclair, 42 Miller Street; Dr. Robert Connell, 1 Minerva Place; Mr. Thomas Brownlie, Timber Merchant, 34 Hill Street, Garnethill.

The President continued his address "On the Astronomy of the Nineteenth Century," at the conclusion of which he received the thanks of the Society, on the motion of Dr. Bryce.

January 10, 1872.—PROFESSOR YOUNG, *Vice-President, in the Chair.*

The following were elected members, viz.:—

Mr. E. L. Mayer, Chemist, 220 Hope Street; Dr. James Dunlop, 17 Carlton Place; Mr. Andrew H. Turnbull, Actuary, Dunard Villa, Dowanhill; Mr. R. Blair Robertson, Chemist, 5 Broomfield Place,

Hillhead; Mr. David Macgregor, Manager, Allander Printworks, Milngavie; Mr. W. B. Crawford, Writer, 97 West Regent Street; Mr. William M'Culloch, 134 Bath Street; Mr. David Bryce, Bookseller, 129 Buchanan Street; Dr. Wolfe, Brandon Place.

Dr. Bryce gave notice that, at next meeting, he would move that a grant be made from the Society's funds in aid of the Expedition proposed to be sent to Africa by the Royal Geographical Society in quest of Dr. Livingstone.

Dr. Robert Bell read a paper "On Biogenesis, or the Development of Life, as Exhibited in Decomposition, Putrefaction, or Decay."

January 24, 1872.—*The* PRESIDENT *in the Chair.*

The following were elected members of the Society, viz.:—

Mr. Mathew H. Cochrane, F.C.S., 108 Peel Terrace; Mr. M. M. Pattison Muir, F.C.S., 12 Woodlands Terrace; Mr. Frederick J. Hallows, 133 West George Street; Mr. Daniel Wilson, Slate and Cement Merchant, 124 Renfield Street; Mr. Mark Bannatyne, Solicitor, 151 West George Street.

On the motion of Dr. Bryce, seconded by Dr. Fergus, the Society voted Twenty Pounds to the Livingstone Search Fund.

Professor Kerr, of King's College, London, delivered a Lecture on the subject of Architecture and Architects in the Metropolis.

The President moved that the thanks of the Society be given to Professor Kerr, which was cordially agreed to.

February 7, 1872.—*The* PRESIDENT *in the Chair.*

The following were elected members, viz.:—

Mr. James N. Dryburgh, 93 North Frederick Street; Mr. John Beckett, Merchant, 16 St. Vincent Place; Mr. Thomas Muirhead, Dyer, 5 Allanton Terrace, Crosshill; Mr. Andrew Hamilton, Surgeon, Thornliebank.

The Society, according to the Rules, confirmed, by a second vote, the grant of £20 to the Livingstone Search Fund.

Mr. John Phillips, C.E., London, read a paper "On the Drainage and Sewerage of Towns."

In the discussion which followed, Mr. Bromhead, Dr. Bell, Mr. M'Adam, Dr. Fergus, Dr. Lyon, and Dr. Gairdner took part. The discussion was adjourned, to be continued in the Sanitary and Social Economy Section. The thanks of the Society were given to Mr. Phillips for his communication.

February 21, 1872.—*The* PRESIDENT *in the Chair.*

Dr. D. Campbell Black was elected a member.

The Council having taken advantage of the re-printing of the Rules of the Society to introduce some amendments, Dr Wallace, Convener of the Committee on Revision, pointed out the principal alterations, and proposed that the Society should now, by a first vote, approve of the draft of Rules as amended.

Dr. Bryce called attention to the propriety of the Society's acquiring the privileges of an incorporate body. He had formerly brought this subject before the Council, and having been authorized to make the requisite inquiries, had learned from a London Barrister that although the Privy Council has ceased to grant Royal Charters, it is still competent to the Society to memorialize the Board of Trade to be made a corporate body. He would now propose that the subject be remitted to the Council, with instructions to consider and report, before printing the amended Rules. Mr. Day and the President expressed their concurrence in the proposal of Dr. Bryce.

Mr. David Mackinlay proposed that the Council should also be requested to consider whether the funds of the Society could be so administered as to admit of the formation of a Reserved Fund to meet future contingencies.

The Society agreed to remit the proposal of Dr. Bryce and that of Mr. Mackinlay to the Council, with instructions to consider and report; and at the same time adopted the motion of Dr. Wallace, by a first vote, approving of the draft of amended Rules, but reserving authority to proceed with the printing till the Council shall have reported on the two subjects of remit.

Mr. Charles Randolph read a paper entitled, "The Projectile Theory as connecting Astronomy with Geology and Geography."

March 6, 1872.—*The* PRESIDENT *in the Chair.*

Mr. James Downie, 69 Ingram Street, was elected a member.

Dr. Wallace moved that the Society, by a second vote, finally adopt the draft of the amended Rules.

After some discussion, the motion was deferred till next meeting; and the Secretary was instructed to obtain twenty-five additional proof-sheets of the amended Rules, for the use of any members who may call for them at the Library.

Dr. Thorpe read a paper "On the Results of the recent Solar Eclipse."

March 20, 1872.—*The* PRESIDENT *in the Chair.*

Mr. David Nicol, Clydesdale Bank, and Mr. George Gardner, Solicitor, were elected members of the Society.

The following reports were received from the Council:—

"The Council took into consideration the remit of the Society with respect to its becoming an incorporate body, by obtaining the necessary powers from the Board of Trade. The Council were of opinion that the time had now come when the Society ought to be invested with the powers of an incorporate body, and agreed to report accordingly to the Society.

"With regard to the remit from the Society to consider the propriety of forming a Reserved Fund, the Council do not find themselves in a position at present to deal with the question, although it has been more or less under their attention for several years. Meanwhile the Finance Committee of the Council exercises a stringent economy in the expenditure of the Society's funds.

"The Council having received representations on the part of a large number of members of the Society, of the unsuitableness of Wednesday evening for the ordinary meetings, came to a unanimous resolution to recommend to the Society to change the night of meeting from Wednesday to Tuesday."

Dr. Bryce moved the adoption of the Council's recommendation that the requisite steps be taken to obtain for the Society the powers and privileges of an incorporate body.

The motion was seconded by Mr. Day.

A motion by Mr. J. Mayer, seconded by Mr. Thomson, that the

expense of obtaining the deed of incorporation be restricted to £20, was negatived.

The motion of Dr. Bryce was approved of.

Mr. James R. Napier gave notice of several motions relating to alterations in the Rules of the Society printed in 1865.

Dr. Wallace moved that the Society, by a second and final vote, approve of the amended draft of the Rules, as laid before the Society by the Council.

Mr. Edmund Hunt moved that, as the Society had decided to take steps for becoming incorporated, the further consideration of the amended draft be deferred until proceedings are taken for carrying out the incorporation.

Mr. James R. Napier seconded the motion.

After some discussion, it was agreed to defer the consideration of the Rules till this day week; to which day the Society resolved that the present meeting, after the reading of the papers announced in the billet, shall stand adjourned.

Mr. James R. Napier, F.R.S., read a paper "On Pressure Logs for Measuring the Speed of Ships."

The paper announced by the President was postponed till the next ordinary meeting on the 10th of April.

March 27, 1872.—The Society met this evening by adjournment, Dr. Young, Vice-President, in the Chair.

The amended draft of Rules having been read by the Secretary,

Dr. Wallace moved that the Society, by a second vote, finally adopt the draft of Rules as amended by the Council.

Dr. Bryce seconded the motion.

Mr. Hunt moved, as an amendment, that the Rules be considered *seriatim*.

Mr. J. Mayer seconded the amendment.

The amendment having been put to the vote, there voted 14 in favour of it, and 16 against it.

The motion was next submitted to the vote, when 17 voted for and 14 against it.

As Rule XIV. provides, first, that a motion for altering the laws of the Society "shall only be agreed to by the votes of two-thirds of the members present;" and, secondly, that "should the motion be negatived, it is lost for that session," the motion was accordingly declared to be negatived and lost for the present session, there not being two-thirds of the votes of the members present in its favour.

April 10, 1872.—The PRESIDENT in the Chair.

The following were elected members, viz.:—Mr. John Adamson, 8 Whitehill Street, Dennistoun; Mr. James Henry Kerr, Sugar Merchant, 20 Dunlop Street; Mr. Robert M'Alley, Manufacturing Chemist, Chemical Works, Falkirk.

The President read a paper "On the Indications we at present possess respecting the Extent which the Meteoric System occupies in the Physical Universe."

April 24, 1872.—The PRESIDENT in the Chair.

Mr. James M'Gregor, Shipowner, 2 Queen's Terrace, and Mr. James M'Ilwraith, Manufacturer, 6 Berkley Terrace, were proposed as members. (Ballot at opening of next session).

Dr. A. K. Irvine read a paper "On a new form of Singing Flame, and its application to a new Miner's Safety Lamp, to Audible Signals, and for Indications of Danger."

Mr. J. R. Napier read a note supplementary to his paper "On Pressure Logs."

Mr. Edward C. C. Stanford exhibited "A new Monochromatic Flame" and "A Chemical Imitation of Lightning."

Mr. Robert R. Tatlock gave in the annual report from the Chemical Section.

REPORT OF THE CHEMICAL SECTION.

THE Council of the Chemical Section are glad to be able to report to the Society that this has been a most successful session. The papers have been both numerous and important, and have, in most cases, elicited discussions of great interest. Eight new Associates have been enrolled. Twelve meetings in all have been held; at which twenty-three papers were read.

The following is a list of the papers communicated:—

1871.

Nov. 6. PRESIDENT'S ADDRESS.

Dec. 4. "On the Analysis of Chrome Ores," by Dr. JOHN CLARK.

„ „ "On a Method of taking the Specific Gravity of Volatile Liquids," by Dr. THORPE, F.R.S.E.

„ 18. "Some Historical Notes of the Discoveries of Dr. Joseph Black," by JOHN FERGUSON, Esq., M.A.

1872.

Jan. 15. "On the Apparent Loss of Alkali in the Soda Manufacture," by JAMES MACTEAR, Esq.

„ 29. "On some Relations between the Atomic Weights and Volumes and the Chemical Activities of some of the Elements," by J. J. COLEMAN, Esq., F.C.S.

Feb. 12. "On the Utilization of the Waste Substances in Gas Liquor," by R. F. SMITH, Esq.

„ 26. "On some Fallacies in the Ordinary Methods of Testing for Certain Poisons," by J. St. CLAIR GRAY, M.D., etc.

March 11. "On Fehling's Copper Solution," by T. L. PATTERSON, Esq., F.C.S.

„ „ "On some Recent Views regarding the Composition of Ultramarine," by E. M. DIXON, Esq., B.Sc.

„ 25. "On the Metallurgy of Lead," by JOHN JEX LONG, Esq.

April 15. "On the Crossness (London) Sewage Works," by Professor GUSTAV BISCHOF.

„ 29. "Notes from the Laboratory of the Andersonian University:—

1. "On the Action of Pentasnephide of Phosphorus on Carbon Tetrachloride."
2. "On the Solvent Action of Nitric Acid on Silver Chloride."
3. "On a Trisodium Phosphate obtained in the Manufacture of Soda," by Dr. T. E. THORPE, F.R.S.E.
4. "On the Action of Dilute Saline Solutions on Lead."
5. "On a Double Sulphide of Gold and Silver," by M. M. PATTISON MUIR, Esq., F.C.S.
6. "On the Zircons of Ceylon," by M. H. COCHRANE, Esq., F.C.S.
7. "On the Magnetic Sand of Mount Etna," by J. B. HANNAY, Esq., F.C.S.

„ „ "On the Estimation of Phosphoric Acid as Ammonio-Magnesian Phosphate;" and,

„ „ "On the Separation of Phosphoric Acid, Oxide of Iron, Alumina, Lime, and Magnesia," by THOMAS R. OGILVIE, Esq.

„ „ "On the part which Iron and Aluminium Oxides play in the Manufacture of Superphosphate; and on the Comparative Value of Mineral Phosphates," by T. L. PATTERSON, Esq., F.C.S.

1872.

April 29. "On Iona Pebbles," and,

„ „ "On the Action of Various Charcoals on Nitrogenous Organic Matter," by E. C. C. STANFORD, Esq., F.C.S.

Mr. G. Irving Dickson gave in the annual report from the Sanitary and Social Economy Section.

REPORT OF THE SANITARY AND SOCIAL ECONOMY SECTION.

THE present session has been in every respect most successful, and its proceedings all along conducted with considerable energy.

As in the previous session, the Sewage question has formed the substance of a large proportion of the Papers and Discussions during the ten meetings of this session, now come to a close, in addition to a special Paper by Mr. John Phillips, C.E., "On the Drainage and Sewerage of Towns," read, by request, before the Parent Society, in presence of a very full meeting of members and of the Section's Associates.

The following is an abstract of the proceedings of the Section:—

1st Meeting.—Introductory Address by its President, Andrew Fergus, Esq., M.D., containing special reference to the Sewage question, and at which meeting the Lord Provost of Glasgow took the Chair.

2nd, 3rd, and 4th Meetings.—Paper by Mr. Alexander Scott on "Comparison of the Vital Statistics of the different Districts of Glasgow for the last two years," illustrated with elaborate and most carefully prepared tables and diagrams.

Very interesting discussions followed the reading of this paper for two consecutive meetings, in which Professor Gairdner, the Medical Health Officer of the City of Glasgow, and other well-known gentlemen, took part; and a strong feeling was expressed by the Section that Mr. Scott's paper should form part of the Society's published *Proceedings* for the year.

The Section also unanimously concurred in being strongly of opinion that the Police Board ought to resume the Sanitary Statistics of the City, as formerly divided into the smaller districts, but which were discontinued in 1869, and also that the different diseases, &c., should be more fully classified than they presently are.

5th Meeting.—Paper by Mr. Robert Symington "On Mr. Strang's Patent Sewage Filter."

6th Meeting.—Discussion on paper read by Mr. John Phillips, C.E., before the Parent Society, "On the Drainage and Sewerage of Towns;" during which the subject was illustrated by relative diagrams, and a strong recommendation was expressed by the meeting that the paper should be published in this year's general *Proceedings* of the Society.

7th and 8th Meetings.—Paper by Mr. Kenneth M. Macleod, the City of Glasgow's Sanitary Inspector, "On the City Sanitary Organization—its Aids and Obstacles." Much satisfaction was expressed at the masterly way in which Mr. Macleod treated his subject; and the discussion was carried over for another fortnightly meeting, when Professor Gairdner added further interest to the paper by a special review of Mr. Macleod's remarks.

9th Meeting.—Paper by Mr. Horatio K. Bromhead, I.A., Secretary to the Glasgow Rate Committee of Fire Insurance Offices, "On a Comparison between the Glasgow Dean of Guild Court and the Metropolitan Board of Works, as affecting the Health of the City and its Security from Fires."

After a most interesting discussion, in which William M'Ewen, Esq., the ex-Dean of Guild, gave very valuable information as to the present constitution and working of the Court,—a strong expression of feeling was shewn by the Section, that the Glasgow Dean of Guild Court should be invested with further powers as to controlling the building operations of the city, not only as to its security from fires, but also as affecting the general health of the city.

10th Meeting.—Paper by Mr. Archibald M'Lay "On System of Farming by Irrigation at Keppoch Farm, near Glasgow."

Mr. W. R. W. Smith supplemented this paper by additional facts which had come under his own observation; and Mr. Ramsay of Kildalton expressed a strong opinion that the pot-ale would be more profitably disposed of in directly feeding cattle; but all present strongly agreed that every effort should be used in preventing its flowing into the streams and sewers of the city.

The Associates of the Section now number thirty, of whom two-thirds joined this Session; and the Council respectfully ask the

general members of the Society to co-operate with them in still adding considerably to their number.

The attendance at the various meetings of the session averaged from fifty to sixty; and the Council hope to report further improvement in this respect during the next session, when some most interesting papers are promised to be read before it.

The Council again respectfully desiderate an increased interest and attendance on the part of the general members of the Society at this Section's meetings, and they beg also to reiterate the hope that they may increase its opportunity of usefulness to young men generally, by inducing their younger friends to join the Section as associates, and by means of which they would be interested in useful subjects, and also become ultimately members of the Parent Society itself.

Mr. Horatio K. Bromhead, A.R.I.B.A., gave in (subsequently) the Report of the Architectural Section, being also the thirteenth annual report of the Glasgow Architectural Society.

Report of the Architectural Section.

In consequence of the additional labours involved by arrangements for accommodating the working of the Society to its new Sectional position in the Philosophical Society of Glasgow, the customary meetings have not been so numerous as usual. Importance has, however, been given to the session by a very interesting lecture by Professor Kerr, of King's College, London.

Mr. C. Douglas was appointed Delegate to attend the R. I. B. A. Conference; and Messrs. A. Thomson and C. Douglas to the Conference of the Architectural Alliance, in London. Both meetings were attended by them, and the reports of the meetings will be found in the Library.

The Library has been considerably enlarged by the addition of a number of valuable works, principally selected in London by a deputation from the Library Committee.

The new rule, No. 4, is now on the point of being acted upon, forty-two Associates having been proposed at last meeting. The Council look upon this movement as one likely to have considerable influence on the future welfare of the profession in Glasgow.

It is anticipated that an Architectual Drawing Class will shortly be formed by the proposed Associates, under the personal assistance of some of the senior members of the Society, and otherwise in con-

nection with the Glasgow Haldane Academy and School of Art. This class may probably resolve itself into a Sketching Class during the summer months.

The Council, therefore, consider themselves entitled to report that the Society still continues to advance in the direction of practical usefulness.

The President intimated that this was the concluding meeting of the session.

ADDITIONS TO THE LIBRARY.

DONATIONS.	Presented by	
Proceedings of the Institution of Mechanical Engineers, . .	The Institution.	
Code Napoleon, ...	Professor Rankine.	2 Vols.
Thermodynamic Theory of Waves of Finite Longitudinal Disturbance, ...	,,	Pamphlet.
Path of Large Fireball of November 14th, 1869, ...	Professor Herschel.	,,
Abstracts of Specifications of Patents applied for from 1854 to 1866, .	Regist. Gen., Victoria.	,,
Patents and Patentees, . . .	Her Majesty's Commissioners of Patents.	,,
Urania. Petri Esseiva Carmen, 1870, ...	Amsterdam Society.	,,
Verhandelingen der Koninklijke Akademie van Wetenschappen, 1870, 1871, ...	,,	
Verslagen en Mededcelingen der Koninklijke Akademie van Wetenschappen, 1870, 1871, .	,,	
Jaarboek van de Koninklijke Akademie van Wetenschappen, voor 1869, 1870, . . .	,,	
Transactions of the Royal Scottish Society of Arts, ...	The Society.	1 Vol.
Papers on the Great Pyramid, .	St. John V. Day.	1 ,,
,, on Patents for Inventions, .	,,	1 ,,
Proceedings of the Berwickshire Naturalists' Club, . . .	The Club.	1 ,,
Statistics of New Zealand, . .	Dr. Bryce.	
Proceedings of the Cleveland Institution of Engineers, . . .	The Institution.	5 Parts.
Statistics of the Colony of Victoria,	The Secretary.	2 ,,
Sessional Papers from the Royal Institute of British Architects, .	The Institute.	11 ,,
Transactions of the Midland Institute of Mining Engineers, . .	The Institute.	5 ,,

Donations.	Presented by	
Transactions of the Odontological Society of Great Britain, . .	The Society.	2 and 5 Parts.
Transactions of the Society of Engineers, ...	The Society.	6 Vols.
Transactions of the North of England Institute Mining and Mechanical Engineers, . . .	The Institute.	
Proceedings of the South Wales Institute of Engineers, . .	The Institute.	3 Parts.
Treatment and Utilization of Sewage at Exeter, 1869, . . .	The Committee.	1 Vol.
On the Narrow Gauge Railways, .	T. Currie Gregory.	Pamphlet.
Journal of the Royal Institution of Cornwall, ...	The Institution.	
Annual Report of the Royal Cornwall Polytechnic Society, . .	The Society.	
Transactions of the Photographic Society, ...	The Society.	12 Parts.
Report of an Inquiry in regard to Schools of Technical Science in certain portions of the United States, ...	St. John V. Day.	Pamphlet.
Proceedings of the Literary and Philosophical Society of Liverpool, ...	The Society.	2 Parts.
Proceedings of the American Literary and Philosophical Society, .	The Society.	3 ,,
Smithsonian Miscellaneous Collections, ...	Smithsonian Institution.	7 Vols.
Smithsonian Contributions to Knowledge, ...	,,	11 ,,
Smithsonian Report for the Year 1869, ...	,,	
Transactions of the Connecticut Academy, ...	The Academy.	2 Parts.
Annual of the Boston Society of Natural History, . . .	The Society.	
Astronomical and Meteorological Observations, United States Observatory, ...	Rear-Admiral B. F. Sands.	1 Vol.
Transactions of the Glasgow Geological Society, 1871, . . .	The Society.	
Annales Meteorologiques de l'Observatoire Académie Royale de Belgique, ...	The Academy.	2 Parts.
Bulletins de l'Académie Royale de Belgique, 1869, 1870, . .	,,	2 Vols.

Donations.	Presented by	
Mittheilungen der Kais. und Königl. Geographischen Gesellschaft in Wien, 1870, ...	The Society.	14 Parts.
Congressional Directory for the Third Session United States, .	,,	
Announcement of the Wagner Free Institute of Science for the Collegiate Year 1870, 1871, .	The Institute.	Pamphlet.
Volume of Pamphlets, 8vo, . .	Dr. J. P. Joule.	
,, ,, 4to, . .	,,	
Transactions of Newcastle-upon-Tyne Chemical Society, . .	The Society.	
A Poor Man's Photography at the Great Pyramid, ...	St. John V. Day.	
Notes on the Antechamber of the Great Pyramid, ...	,,	
On an Equal Surface Projection for Maps of the World, . . .	,,	
Examination of Mr. Gillespie's Argument for a Great First Cause,	T. S. B.	
Patent Office Reports for 1867, 1868, United States, . . .		8 Vols.
On a Specimen of Acanthodes Wardii, ...	J. Thomson.	Pamphlet.
Memoires de la Société des Sciences Physiques et Naturelles de Bordeaux, ...	The Society.	2 Parts.
Proceedings of the Bristol Naturalists' Society, . . .	The Society.	
Journal of Anthropology, . .	The Society.	
Proceedings of the Liverpool Geological Society, ...	The Society.	1 Vol.
Lectures delivered at Melbourne, .	J. R. Napier.	Pamphlet.
Journal of the Royal Dublin Society,	The Society.	5 Parts.
Processen Verbaal van de Gewone Vergaderingen der Koninklijke Akademie van Wetenschappen 1871, ...	Amsterdam Society.	
Patents and Patentees for 1869, Colony of Victoria, . . .	Regist. Gen., Victoria.	
Institute of Mining Engineers, .	The Institute.	6 ,,
Memoirs of the Royal Astronomical Society, ...	Professor Grant.	
Jornal de Sciencias Mathematicas, &c., of Academy of Sciences, Lisbon, ...	The Academy.	4 Vols.
Antidote against the Unscriptural and Unscientific Tendency of Modern Geology, . . .	P. M'Farlane.	1 ,,

Donations.	Presented by	
Works of Architecture, by Robert and James Adams, . . .	John Napier.	10 Parts.
Serrurerie et Fonte da fer Récemment Exécutées, by Thiollet, .	,,	1 Vol.
Reports on the Total Solar Eclipse of December 22, 1870, . .	Rear-Admiral B. F. Sands.	Pamphlet.
Bulletins de la Société Chimique, second series, ...	The Society.	2 Vols.
United States Coast Survey, . .	Prof. W. J. M. Rankine.	5 ,,
Proceedings of the Royal Society of Edinburgh, ...	The Society.	4 Parts.
Transactions of the North of England Institute of Mining Engineers, ...	The Institute.	1 Vol.
Transactions of the Botanical Society of Edinburgh, . .	The Society.	
Proceedings of the American Pharmaceutical Association, . .	The Association.	1 ,,
Pamphlets on Various Subjects, .	Elias Loomis.	7 Pamphlets.
Annual Report of the Royal Cornwall Polytechnic Society, . .	The Society.	
Proceedings of the South Wales Institute of Engineers, . .	The Institute.	
Journal of the Royal Institution of Cornwall, ...	The Institution.	
Anaxagoras og Atomistiken. Kritiske Bemerkninger af M. J. Monrad, ...	University of Christiania.	Pamphlet.
Nogle Strobemærkninger om Bevidsthedens Væsen, m. M. Af C. Winter-Hjelm, . . .	,,	,,
Om den Kyrenaiske Skole, Navnlig Annikeris og Theodoros. Af Dr. Ge. V. Lyng, ...	,,	2 ,,
Forhandlinger i Videnskales-Selskabet i Christiania, 1869, 1870, .	,,	2 Vols.

Books Bought.

Descent of Man, and Selection in Relation to Sex. By Charles Darwin. 2 vols. 8vo. 1871.

Keramic Gallery. By William Chaffers. 2 vols. 8vo. 1872.

History of the British Hydroid Zoophytes. By Thomas Hincks, B.A. 2 vols. 8vo. 1868.

Genesis of Species. By George Mivart, F.R.S. 1 vol. 8vo. 1870.

Zoological Record. Edited by Alfred Newton, M.A., F.R.S. 1 vol. 8vo. 1870.

Hours of Exercise in the Alps. By John Tyndall, LL.D., F.R.S. 8vo. 1871.

Five Great Monarchies of the Ancient Eastern World. By George Rawlinson, M.A. 3 vols. 8vo. 1871.

Scrambles amongst the Alps, in the years 1860-1869. By Edward Whymper. 8vo. 1871.

Narrow Gauge Railways. By C. E. Spooner, C.E., F.G.S. 8vo. 1871.

Treatise upon Terrestrial Magnetism. 8vo. 1871.

Volcanoes: the Character of their Phenomena, their Share in the Structure and Composition of the Surface of the Globe, and their Relation to its Internal Forces. By G. Poulett Scrope, F.R.S., F.G.S. 8vo. 1872.

Journeys in North China, Manchuria, and Eastern Mongolia, with some Account of Corea. By the Rev. Alexander Williamson, B.A. 2 vols. 8vo. 1870.

The Antiseptic System: a Treatise on Carbolic Acid and its Compounds. By Arthur Ernest Sanson, M.D. 8vo. 1871.

Lives of Boulton and Watt. By Samuel Smiles. 8vo. 1866.

Egypt of the Pharaohs, and of the Kedive. By F. Barham Zincke. 8vo. 1871.

Birds of the West of Scotland, including the Outer Hebrides. By Robert Gray. 8vo. 1871.

The Ibis, Journal of Ornithology. Edited by Osbert Salvin, M.A., F.L.S., F.Z.S. 8vo. 1871.

History of Painting in North Italy. By J. A. Crowe and G. B. Cavalcaselle. 2 vols. 8vo. 1871.

Essays on Cathedrals by Various Writers. Edited by the Very Rev. J. S. Howson, D.D., Dean of Chester. 8vo. 1872.

Memoir of John Elder, Engineer and Shipbuilder. By W. J. Macquorn Rankine. 12mo. 1871.

The Origin of Civilization and the Primitive Condition of Man. By Sir John Lubbock, Bart., M.P., F.R.S. 8vo. 1870.

An Essay on the Druids, the Ancient Churches, and the Round Towers of Ireland. By the Rev. Richard Smiddy. 12mo. 1871.

The Subterranean World. By George Hartwig. 8vo. 1871.

Hand-book of Chemistry. By Leopold Gmelin. 2 vols. 8vo. 1872.

Locomotive Engineering and the Mechanism of Railways. By Zerah Colburn. 2 vols. royal 4to. 1871.

The Works of Sir William Jones, containing the whole of the Asiatic Researches. 8 vols. 4to. 1801.

Traité de Chimie. Par M. Dumas. 9 vols. 8vo. 1846.

Course of Lectures on Natural Philosophy and the Mechanical Arts. By Thomas Young, M.D. 2 vols. 4to. 1807.

On Naval Timber and Arboriculture. By Patrick Matthew. 1831.

National Gazetteer of Great Britain and Ireland. Edited by N. E. S. A. Hamilton, F.R.S.L. 7 parts. 1872.

Glasgow, Ancient and Modern. Edited by J. F. S. Gordon, D.D. 2 vols. 8vo. 1872.

Notes on the Natural History of the Strait of Magellan and West Coast of Patagonia. By Robert O. Cunningham, M.D., F.L.S. 8vo. 1871.

Annales du Conservatoire Imperial des Arts et Métiers. 8 tomes 8vo. 1867.

Bulletin de la Société Géologique de France. 8vo. 1870.

The Sewage Question. By Andrew Fergus, M.D. Pamphlet. 1872.

Rude Stone Monuments in all Countries, their Age and Uses. By James Ferguson, D.C.L., F.R.S. 8vo. 1872.

A New Sea and an Old Land. By W. G. Hamley. 8vo. 1871.

The Beginning: Its When and Its How. By Mungo Ponton, F.R.S.E. 8vo. 1871.

Mechanical Philosophy. By John Robison, LL.D. 4 vols. 8vo. 1822.

Traité de Géométrie Elementaire. By Eugene et Ch. de Comberousse Rouché. 8vo. 1868.

Lighthouse Illumination. By Thomas Stevenson, F.R.S.E., M.I.C.E. 8vo. 1871.

Primitive Culture of Man. By Edward B. Taylor. 2 vols. 8vo. 1871.

Insects at Home; being a Popular Account of British Insects. 8vo. 1872.

Mineral Statistics of the United Kingdom of Great Britain and Ireland. By Robert Hunt. 8vo. 1872.

Lectures on the Sciences of Languages. By Max Müller, M.A. 2 vols. 8vo. 1871.

Handbook of Practical Telegraphy. By R. T. Culley. 8vo. 1871.

An Exposition of Fallacies in the Hypothesis of Mr. Darwin. By C. R. Bree, M.D. 8vo. 1872.

Chefs-d'Œuvre of the Industrial Arts. By Philippe Barty. 8vo. 1869.

Observations on some Interesting Phenomena in Animal Physiology. By John Graham Dalzell. 8vo. 1814.

Campi Phlegræi: Observations on the Volcanoes of the Two Sicilies. Folio. 1776.

Memoirs of the Geological Survey of Great Britain and of the Museum of Practical Geology. By Robert Hunt, F.R.S. 8vo. 1872.

Report of the Commissioners appointed to inquire into the several Matters relating to Coal. 3 Parts. 1871.

Principles of Psychology. By Herbert Spencer. 2nd edition, 8vo. 1870.

Illustrations of Natural History, wherein are exhibited upwards of Two Hundred and Forty Figures of Exotic Insects. By D. Drury. 3 vols. 4to. 1770.

Illustrations of British Entomology; or, a Synopsis of Indigenous Insects. By James Francis Stephens, F.L.S. 12 vols. 8vo. 1828.

Estuary of the Forth and Adjoining Districts, viewed Geologically. By David Milne Home of Wedderburn. 8vo. 1871.

Treatise on Valve-Gears, with Special Consideration of the Link-Motions of Locomotive Engines. By Dr. Gustav Zeuner. 8vo. 1869.

A Journey Across South America, from the Pacific Ocean to the Atlantic Ocean. By Paul Marcoy. 2 half-vols. 4to.

Memorials of the Old College of Glasgow. 4to. 1871.

Manual of Colours and Dye Wares. By J. W. Slater. 12mo. 1870.

Mathematical Papers of the late George Green. Edited by N. W. Ferrers, M.A. 8vo. 1871.

Cook's Voyages to the Pacific Ocean, in the years 1776–1780. 8 vols. and 1 vol. plates. 4to. 1784.

Antiquities of the State of New York, with a Supplement on the Antiquities of the West. By E. G. Squier, M.A. 8vo. 1851.

Catalogue of Scientific Papers (1800-1863), compiled and published by the Royal Society of London. 4to. 1871.

Journal of the Royal Geographical Society. 4th vol., 8vo. 1870.

Principles of Mechanism. By Robert Willis, M.A., F.R.S. 8vo. 1870.

Borough of Birmingham: Report of the Sewage Inquiry Committee, presented to the Council on the 3rd of October, 1871. 8vo. 1871.

Royal Institution: its Founder and its First Professors. By Dr. Bence Jones. 8vo. 1871.

The Student's Manual of Geology. By J. Beete Jukes, M.A., F.R.S. Edited by Archibald Geikie, F.R.S. 8vo. 1872.

The Statesman's Year-Book. Handbook for Politicians and Merchants, for the year 1872. By Frederick Martin. 12mo. 1872.

Elementary Treatise on Natural Philosophy. By A. Privat Deschanel. 3 parts, 8vo. 1870.

Dictionary of Chemistry (Supplement). By Henry Watts. 8vo. 1871.

Report of the British Association for the Advancement of Science for the years 1870-71. 8vo. 1872.

Egypt's Place in Universal History: an Historical Investigation, in Five Books. By Christian C. J. Bunsen, D.Ph., D.C.L. 5 vols. 8vo. 1867.

PERIODICALS RECEIVED IN THE LIBRARY.

Weekly Periodicals.

Architect.
Athenæum.
Builder.
Building News.
Engineer.
Engineering.
Mechanics' Magazine.
Polytechnisches Journal.
Annales Industrielles.
Chemical News.
Comptes Rendus.
Journal of the Society of Arts.
Les Mondes.
Nature.
Notes and Queries.
Pharmaceutical Journal.

Monthly.

Annals and Magazine of Natural History.
American Journal of Science and Arts.
Annalen der Physik und Chemie.
Annalen der Chemie und Pharmacie.
Annales des Ponts et Chaussees.
Annales de Chimie et de Physique.
Geological Magazine.
London, Edinburgh, and Dublin Philosophical Magazine.
Journal of Botany.
,, Chemical Society.
,, fur Praktische Chemie.
,, de Pharmacie et de Chimie.
The Country Gentleman's Magazine.
Monthly Microscopical Journal.
,, Notices of the Royal Astronomical Society.
Abstracts of the Proceedings of the Geological Society of London.
Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin.

QUARTERLY.

Journal of the Statistical Society.
,, of Anthropology.
,, of Iron and Steel Institute.
Annales des Mines.
Quarterly Journal of Science.
Quarterly Journal of Microscopical Science.
English Botany (in Quarterly Numbers).
Bulletin de la Société Chimique de Paris.

PROCEEDINGS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW.

SEVENTIETH SESSION.

XI.—*On some Evidences as to the Very Early Use of Iron.* By Mr. St. John Vincent Day.

Read before the Society, December 4, 1872.

In a former paper, read to this Society during the Session 1870-71, and published in the *Proceedings*,* I brought together a variety of evidence, contemporary with the instances noticed, proving that iron, in one of its various states at least, was well known to, and used by one of the most ancient nations of the world during the very earliest ages of which we have record. Since reading the paper alluded to, I have instituted further investigation of the subject, and as this has been productive in bringing to light additional evidence, it has occurred to me that a statement thereof may prove useful and interesting to members of this Society who may be engaged in exploring the areas of remotest archæology.

The more extended our enquiries become concerning the state of some, at least, of the arts in ancient times, so have they always resulted in indicating an ever-increasing rise towards excellence in the ultimate product, in proportion as the examples cited belong to ages more remote from our own times. This assertion may doubtless be questioned by some; yet, as a point upon which those who have investigated high archæology appear to be agreed, that just mentioned seems to stand out as settled. Many proofs of this may be found, but it is sufficient to signalize at present one or two—such as the Architecture and Sculpture of Proto-Egypt, Assyria, India, and Greece—which, it is well known, have never been equalled in subsequent ages. That the artificers employed in executing the most ancient examples of mechanical execution in Egypt were possessed of iron tools, (and, as I shall presently shew, almost certainly steel,) my former paper was, in part, directed to prove; but not the least significant question naturally suggesting itself, as an outcome of the already mentioned fact, is, From what source

* Vide *Proc. Phil. Soc., Glasgow*, vol. vii., pp. 476-488.

was the iron obtained? With regard to this question there has hitherto been some difficulty in accounting for the origin of the iron so used by the Proto-Egyptian artificers; and failing better evidence, Mr. Basil Cooper, in his learned dissertation,* has, at the suggestion of Mr. Pengelly, been led to conclude in favour of "meteoric iron;" Sir John Lubbock, too, leaves room for that to be inferred from his researches in regard to some countries.† In my former paper I pointed out that this explanation was scarcely well founded; and recent evidence of what is ascertained as to the existence of iron in the Egyptian limestone renders such a conclusion still more improbable.

A learned friend,‡ who has spent some time in Lower Egypt, assures me that the iron of the limestone there, becomes washed out of the matrix, filters through and accumulates in hollow spaces or fissures, just as it does in South Africa on the eastern frontier of Cape Colony, where the Kaffirs, or more properly the Amakosas or Amapondas, have been famous from time immemorial for taking some of the flaky ironstone formed as aforesaid, and, after heating it in little charcoal fires, with hammers and anvils of stone, beating out good malleable iron heads for their "assegais." I am further informed that the late Colonel Dundas, R.A., sometime Superintendent of Woolwich Arsenal, had been among those Kaffirs, and seen them forge their "assegais"|| heads in the manner I have described.

It is not, of course, presumed to assert that what obtains at Cape Colony in our own day is by any means a proof that the Proto-Egyptians adopted a similar course of manufacturing iron when in possession of like materials for doing so, but it is put forward not only as a possible but much more probable theory than that which supports meteoric iron as the source from which they made their tools and weapons.

Whilst having had occasion to mention the native iron-makers

* *Trans. Devonshire Association*, 1868. † See Appendix A.

‡ The Scottish Astronomer-Royal.

|| The "assegai" is between an arrow and a spear—a javelin for throwing by hand, consisting of a thin tapering shaft of wood or reed, and an iron head, shaped thus, three to five inches long. Therewith the Kaffir kills his prey from a distance, and stabs his enemy close by; he also uses it as a knife to cut up a bullock for eating. The blades are said to have a slight skew on the central axis, to make the shaft rotate as it flies through the air. When the Kaffirs go out to fight, each man has a bundle of these iron-headed assegais, with fine cutting blades too.

of Cape Colony, I cannot deny myself from referring to the bearing of the evidence there obtained against the modernly assumed law of an universal succession of stone, bronze, and iron ages. If that be a law at all for any one country, and against which there is a powerful testimony, it is certainly not the law at Cape Colony; for there are the savages now, never known to have worked copper or bronze, but who are nevertheless most excellent workers in iron.

By another very ancient nation, too, we have substantial proof that iron was used—namely, the Chaldeans! for although the examples which have come down to us are small and few, yet that is not remarkable when we remember the quickly oxidizing character of this metal, in comparison with copper, silver, gold, &c. The Rev. Henry Rawlinson, in his *Five Great Monarchies*, vol. i., pages 98, 99, says, in reference to the metallurgy of the Chaldeans, "Metal seems to be scarce, and not many kinds are found; there is no silver, zinc, or platinum, but only gold, copper, tin, lead, and *iron*." . . . "*Iron*, as already observed, is extremely uncommon, and, when it occurs, is chiefly used for the rings and bangles which seem to have been the favourite adornment of the people." When, however, we do find relics of iron manufacture among the ruins of the Proto-Chaldean kingdom, which had its period of chief activity and importance between 2000 and 1500 B.C., few though they be, and these very few doubtless belonging to a large number of original examples, lost through oxidation—coming down to us now in tangible form—we are led to infer that these, such as they are, have reached us owing to some peculiarities of position, which have ensured their dryness, or otherwise interfered with the natural tendency to rapidity of oxidation attaching to iron.

After the decay of the Proto-Chaldean kingdom, when the seat of Mesopotamian empire moved north-westward up the Tigris, and the Assyrians flourished—*i.e.*, from about B.C. 1600 to B.C. 625—we find them also as workers and users of iron—on which the following testimony of Mr. Layard is conclusive. Speaking of the "finds" in the north-west palace of Nimroud (the ancient Calah), he says,* "As we approached the floor (*i.e.*, of Chamber 1), *a large quantity of iron was found* amongst the rubbish, and I soon recognized in it the scales of the armour represented on the sculptures. Each scale was separate and of *iron*, from two to three inches in length, rounded at one end and square at the other, with a raised or embossed line in the centre. The

* *Nineveh and its Remains*, vol. i., p. 341, *et. seq.*

iron was covered with rust. . . . As the earth was removed, other portions of armour were found, some of copper, others of *iron*, and others of iron inlaid with copper. At length a perfect helmet (representing in shape and in the ornaments the pointed helmet represented in the bas-reliefs) was discovered. When first separated from the earth it was perfect, but immediately fell to pieces. I carefully collected and preserved the fragments, which were sent to England. The lines which are seen round the lower part of the pointed helmets in the sculptures are thin strips of copper, inlaid in the iron. Several helmets of other shapes, some with the arched crest, were also uncovered, but they fell to pieces as soon as exposed, and I was only able, with the greatest care, to gather up a few of the fragments which still held together; for the iron was in so complete a state of decomposition that it crumbled away on being touched." In regard to this quotation, it is almost unnecessary to remark, that the correspondence of the armour structurally, with that shown by the bas-reliefs, is as sure a proof as we can expect ever to come across of the contemporaneousness of both; and, holding in view that structural character, it shows that the Assyrians, in the early period of their history, had acquired very considerable proficiency in the execution of complicated iron work, requiring, too, very excellent iron to submit to the various contortions and strains inherent to shaping it as armour for the human body. One of the sources from which the Assyrians obtained this iron was most probably the Tiyari mountains. Mr. Layard, in regard to the metallic deposit there, says,* "In the Tiyari mountains, particularly in the heights above Lizan, and in the valley of Berwari, mines of *iron*, lead, copper, and other minerals abound. Both the Kurds and Chaldeans make their own weapons and implements of agriculture, . . . collecting the ores, which are scattered in the declivities or brought down by torrents." Further on he remarks,†—"Their mountains furnished a variety of minerals—silver, *iron*, copper, and lead, and perhaps even gold." "Iron, the most useful of all the metals, was the one which most abounded, and which could most easily be procured, as soon as the process of extracting it from the ore was known. I have observed that it is found in great quantities, scattered on the sides of mountains, three or four days' journey from Mosul. Amongst the objects of tribute enumerated in the Statistical Table of Karnak, *iron* is mentioned as brought to the Egyptians almost exclusively by the inhabitants

* *Nineveh and its Remains*, vol. i., pp. 223, 224, 5th ed., 1854.
† *Ibid*, vol. ii., p. 415, 6th ed., 1854.

either of Assyria, Persia, or of the countries immediately adjacent, by the Tahai, the Rutenu, and the Asi. It was generally exported in the form of bricks or pigs, but also occasionally in the ore."

In his other work,* Layard speaks still more assuringly as to the degree of skill acquired by, and the extent to which iron was used by the Assyrians, thus†:—"It would appear that the Assyrians were unable to give elegant forms or a pleasing appearance to objects in iron alone, and that consequently they frequently overlaid that metal with bronze, either entirely or partially, by way of ornament. Numerous interesting specimens of this nature are included in the collection of the British Museum." "The feet of certain tripod stands‡ found at Nimroud—these consisting of a

* *Nineveh and Babylon.* † p. 191 *et. seq.* London, 1853.

‡ Speaking of one of these, Dr. Percy says:—"This was a very remarkable specimen. It was a small casting, in the shape of the fore-leg of a bull. It formed the foot of a stand, consisting of a ring of iron, resting on three feet of bronze. It was deeply corroded in places, and posteriorly was fissured at the upper part. A section was made, which disclosed a central piece of iron, over which the bronze had been cast. At the upper part, where it had been broken off, the iron had rusted, and so produced the crack above mentioned. The casting was sound, and the contact perfect between the iron and surrounding bronze. It was evident on inspection that the bronze had been cast round the iron, and that the iron had not been let into the bronze; and in this opinion I am confirmed by Mr. Robinson of Pimlico, who has had considerable experience in bronze casting.

	Composition.
Copper, ...	88·37
Tin, ...	11·33
	99·70.

Some interesting considerations are suggested by this specimen. The iron was employed either to economize the bronze, for the purpose of ornament, or because it was required in the construction. If the former, iron must have been much cheaper than bronze, and, therefore, probably more abundant than has been generally supposed. No satisfactory conclusion can be arrived at on this point from the fact, that bronze antiquities are much more frequently found than those of iron; for the obvious reason, that bronze resists, much better than iron, destruction by oxidation. Although I think there are reasons for supposing that iron was more extensively used by the ancients than seems to be generally admitted, yet, in the specimens in question, it appears to me most probable, that the iron was used because it was required in the construction. And if this be so, the Assyrians teach a lesson to many of our modern architects and others, who certainly do not always employ metals *in accordance with their special properties.* The instrument under consideration, it will be borne in mind, was one of the feet of a stand composed of an iron ring resting upon vertical legs of

circular ring raised upon feet, to hold jars and vases—are frequently represented on the bas-reliefs, as shown in M. Botta's large work." "The ring was of iron, bound in some places with copper, and the feet, partly of iron and partly of bronze, ingeniously cast over it;" * whilst "the iron enclosed within the copper has not been exposed to the same decay as that detached from it, and will still take a polish."† Then, concerning the nationality to which the artificers of these specimens belong, Layard suggests,—"From the Egyptian character of the designs, ... it may be inferred that some of them were not Assyrian, but had been brought from a foreign people," who, he appears, and very satisfactorily, to conclude, were the Sidonians and other inhabitants of the Phœnician coast, the most renowned workers in metal in the ancient world, and whose intermediate position between the two great nations, he considers, may have been the cause of the existence of a mixed art among them. Of the other "finds" at Nimroud, Layard mentions many in iron. For example, he says, "Around the vessels I have described were heaped arms, remains of armour, *iron instruments*."‡ . . . "The arms consisted of swords, daggers, shields, and the heads of spears and arrows, which, being chiefly of *iron*, fell to pieces almost as soon as exposed to the air. A few specimens have alone been preserved, including the head of a weapon resembling a trident, and the handles of some of the swords."§ . . . Speaking of the bronze shields found also at Nimroud, Layard says, "the handles are of iron."‖ Again, he mentions that "a number of thin iron rods, adhering together in bundles, were found amongst the arms. They may have been the shafts of arrows;"¶ and of arrow heads, he tells us that "several such barbs, both of iron and bronze, have been found in Assyrian and Babylonian ruins, and are preserved in the British Museum;"** whilst, as the crowning point to the discussions bearing on the ancient use of iron, he adds—"Amongst

bronze. A stand of this kind must have been designed to support weight, probably a large cauldron; and it is plain that the ring portion should therefore be made of the metal having the greatest *tenacity*, and the legs of metal adapted to sustain *vertical or superincumbent weight*. Now, this combination of iron and bronze exactly fulfils the conditions required. I do not say that a ring of bronze might not have been made sufficiently strong to answer the purpose of the ring of iron; but I do say that, in that part of the instrument, iron is more fitly employed than bronze. Moreover, the contrast of the two metals, iron and bronze, may also have been regarded as ornamental."

* *Nineveh and Babylon*, p. 178. † *Ibid.* p. 191.
‡ *Ibid*, p. 194, 195.
§ *Ibid.* ‖ Ibid. ¶ *Ibid.* ** *Ibid.*

the *iron* instruments were the head of a pick, a double-handled saw (about 3 feet 6 inches in length), several objects resembling the heads of sledge-hammers, and a large blunt spear-head, such as we find from the sculptures were used during sieges to force stones from the walls of besieged cities.'* It should not be passed over

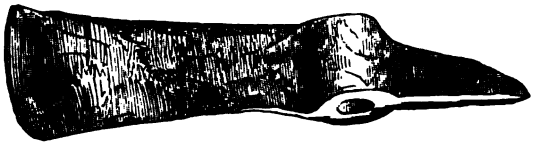


Fig. 1. An Iron Pick, from Nimroud.

Fig. 2. Half of a Double-handled Saw, from Nimroud.

either, that according to Herodotus and Pausanias, the Lydian King Alyattes, who died about 570 B.C., amongst other offerings, dedicated to his god an inlaid *iron* saucer.

As I have previously dwelt upon the use of iron among the early Egyptians, and have produced both literary as well as material evidence of that use, it is only necessary to cite the conclusion at which Mr. Basil Cooper arrived, in respect of the antiquity of that use by them, from studies of a different order to those which I myself have more particularly pursued. He says,—"Turning to Egypt, . . . we find that there also, as well as on the classical soil of Greece and Rome, the origin of the art of working in iron is pushed back into the mythological and pre-historic age. We have no reason to doubt the testimony of Diodorus, when he repeats that the Egyptians assigned this invention also, as well as all the other more important arts of life, to their great national culture divinity, Osiris." This at least implies that it was known amongst them from time immemorial.

I have already shewn, in my previous communication to this Society,† that the use of iron is identified with the time of erecting

* *Nineveh and Babylon*, pp. 194, 195.

† Vide *Proc. Phil. Soc. Glasgow*, vol. vii., p. 481, *et. seq.*

the oldest known Egyptian monuments, which, indeed, are by universal testimony proved to be the oldest on the earth; and in view of the abundant evidence of a high yet peculiar civilization prevailing in that land in the remotest ages we can fathom, it has by many persons been assumed that most of the arts and sciences were spread therefrom among other peoples.* It is possible, nay, it is somewhat probable, that the knowledge possessed by the Proto-Egyptians of working in iron may have been brought with them from the Mesopotamian plain, whence they travelled westwards until finding the Nile delta for a resting-place. Yet, as bearing against that probability, we must remember that in the broad alluvial expanse situate between the Euphrates and Tigris they had not materials to work in, which demanded the use of iron or steel tools, such as they found when meeting with the Mukattam and nummulite limestones, syenites, porphyries, diorites, and granites of the Nile valley. Yet, again, as strengthening the probability, we have the well ascertained fact, that in the same alluvial soil the Proto-Chaldeans—and subsequently to them the Assyrians—practised the art of working in numerous metals, including iron. Whilst, then, we have not at present sufficient knowledge to enable us to link together the chain of testimony by which to prove the centre at which iron smelting took its rise, but finding it prominently referred to in most ancient literature, and in none more decidedly than the earliest parts of the Hebrew Scriptures, we shall probably not greatly err in venturing to suggest that it was first practised in the highlands of Central Asia, prior to the great breaking up of the human family, once cradled there, and their march north, south, east, and west, to inhabit the earth; after which, it is not at all improbable that, from various causes—and none less so than the non-existence of iron ore in some of the regions to which they travelled—this, as well as others of the arts, fell into disuse among some sections of the divided people, until in their progress further and further from their cradle-land they met with the ore, and, either from recollection of the past, or by being subjected to conditions requiring the use of metals, their attention was directed to it. At such times the practice of reducing the ores and working in the metals would be revived, perhaps rediscovered — these independent occasions then corresponding to the alleged first

* Dr. Birch, of the British Museum, in writing me on the 27th October, 1871, says,—"Under the Roman dominion in Egypt, 'iron nails are said to have been found in coffins.'"

discovery of iron smelting ascribed by different peoples to themselves, such as we find it recorded in their traditions and literature which have reached us.

We may now pass down the stream of time to the Greeks, and in the records of which people we find striking evidence of their acquaintance with the use and manufacture of iron. Indeed, as characteristic of that one-sided conceit which showed itself prominently through so many phases of the Greek mind, it is not surprising that, terming other nations as "barbarians," the Greeks should attribute to themselves the discovery of iron, after the manner handed down to us in the Pelasgic tradition, connecting it with the Idœan Daktyls* (Δάκτυλοι Ἰδαῖοι), professing that that discovery took place on the occasion of the burning of the forest which crowned their native mountain. The date of that conflagration, according to the Parian chronicle and other authorities, is the fifteenth century before the Christian era, or more precisely B.C. 1462.

* The following letter was addressed to me some time since. To the philologer it will prove interesting and important:—

"CLYDESDALE VILLA, TOR PARK, TORQUAY,
26th January, 1871.

"DEAR SIR,

"I have much pleasure in forwarding my paper on 'Antiquity of the Metals, and especially of Iron, amongst the Egyptians,' as requested. Since reading my paper, one or two thoughts bearing on the tradition about the Idœan Daktyls have struck me, which may possibly nterest you.

"Ida is the Phœnician and Hebrew word יד (YAD) = Hand, of which the Daktyls or Fingers are the Peaks. The Mythological Daktyls are the Peak *people* (of whom the priests of the mountain Mother Cybele, called Daktyls, claimed to be the representatives), which wrought the mines of the mountain. I am disposed to connect the Ida-feld and Asi of the Norse Mythology, and even the German word Eisen* (our Iron), nay, more, the Greek σίδηρος, with the old Culture Myth of the Daktyls. The last is simplified by a reference to the Coptic ⲟⲩϫ = Hand. The first letter shaped like the Greek Sigma, and called by a name derived from it—the Tsima—seems, to judge from that name, to have sounded Ts, although, possibly, it may have had the sound of the French J, or of our own Z in the word Azure. The last, called the Djandja, sounded like our J. Hence Tsij or ẑij = σίδ, of which σίδηρος would be simply an adjectival form = Idœan (sc. metal).

"The Phrygian Ida must be that meant in the original tradition. Its two principal Peaks were Gargaron, with its Zeus cultus, known already to Homer, and Kotulos. The former is probably a Phœnician word, like the

* Gothic, AIZ = AID-s, the s being flexion.

Nor is the foregoing a solitary instance of literary testimony amongst the early Greeks as to their acquaintance with the metals. The shield of Achilles, described as of such excellent and intricate workmanship, in the Homeric epic, is another instance; whilst to Aristotle himself we are indebted for nothing less than an actual account of the mode in which the Greeks made steel in the 4th century B.C., proving them at the same time to have been acquainted with iron in the *molten state* also. The passage has been translated by Mr. Martin Lister,* and reads thus:—"Wrought iron itself may be cast so as to be made liquid, and to harden again; and thus it is they are wont to make steel; for the scoria of iron subsides, and is purged off by the bottom; and when it is often defecated and made clean, this is steel. But this they do not often, because of the great waste, and because it loses much weight in refining; but iron is so much the more excellent the less recrement it has." Somewhat obscure, no doubt, this Aristotleian passage is; yet, when we carefully sift the nature of that which it conveys to us, it is replete with significance, as describing very briefly and imperfectly, no doubt, what can scarcely be any other than a process of making steel, used in Germany certainly as late as the sixteenth century, that being proved from Agricola.† The same process, too, was in use in the Island of Elba as late as

name of the mountain, and answers to the Galgala, by which the LXX. transcribe the Gilgal of the Bible. The name means Circle, and seems to point to a Keltic Circle of Stones, and to the worship of a Jupiter Tonans, to whom would answer the Damnamenos (=the Hammerer — Thor), whom we find among the Three Original Daktyls. Kotulos is Greek, for the more common Kotule. The form Kotulos is found in a work ascribed to Homer—meaning Cup—and at once suggests the Crater of a Volcano, or Burning Mountain, as the tradition represents Ida to have been. This Peak would best answer to Kelmis, the Smelter, a personification of the Smelting-furnace, whence the molten metal flowed. As to the third Peak, to answer to Akmōn, the only suggestion I have to offer is, that an Akmōn is mentioned as the father of the Phrygian King Mugdōn, spoken of in *Il.* iii. 184–190.

"If these rough jottings be deemed worth your notice, they are much at your service.

"Yours, DEAR SIR, very truly,

"BASIL H. COOPER.

"ST. JOHN VINCENT DAY, Esq."

* *Phil. Trans.*, vol. iii., Abridged ed., p. 571.

† *De Re Metallica*, lib. 9. Agricola describes the process thus:—"Make choice of iron which is apt to melt, and yet hard, and may easily be wrought with the hammer; for although iron, which is made of vitriolic ore, may melt, *yet it is soft*, or brittle, or eager. Heat a parcel of such iron red-hot, and cut

1683, A.D. (Kircher); and this place has been famous, from the time of the Roman Empire, for the production of steel.* From all the evidence we are led to infer that probably for about 3,700 years the production of steel, by immersing bars of wrought iron into a molten carburet of iron, was practised among the nations in their progress westward—brought from Egypt, almost to a certainty, by the Greeks—established by them in the Mediterranean countries they occupied—whence it was acquired by the Romans, and by them introduced throughout their vast western empire.

From what Pliny had observed, and has handed down to us (lib. 34, c. 14),† it appears that the Romans in his time—*i.e.*, in the earlier

it into small pieces, and then mix it with a sort of stone which easily melts; then set in the smith's forge, or hearth, a crucible, or dish of crucible metal, a foot and a-half broad, and a foot deep; fill the dish with good charcoal, and compass the dish about with loose stones, to keep in the mixture of stone and pieces of iron. As soon as the coal is thoroughly kindled, and the dish red-hot, give the blast, and let the workman put on by little and little, all the mixture of iron and stone he designs. When it is melted, let him thrust into the middle of it three or four or more pieces of iron, and boil them therein five or six houts with a brisk fire; and, putting in his rod, let him often stir the melted iron, that the pieces may imbibe the smaller particles of the melted iron, which particles consume and thin the grosser ones of the iron pieces, acting like a ferment to them, and making them tender. Let the workman now take one of the pieces out of the fire and put it under the great hammer, to be drawn out into bars and wrought; and then, hot as it is, plunge it into cold water. Thus tempered, let him again work it on the anvil, and break it; and viewing the fragments, let him consider whether it looks like iron in any part of it, or be wholly condensed and turned into steel. Then let the pieces be all wrought into bars; which done, give a fresh blast to the mixture, adding a little fresh matter to it, instead of that which had been drunk up by the pieces of iron, which will refresh and strengthen the remainder, and make still purer the pieces of iron again put into the dish; every which piece let him, as soon as it is red-hot, beat into a bar on the anvil, and cast it, hot as it is, into cold water. And thus iron is made into steel, which is much harder and whiter than iron."

* *Phil. Trans.*, vol. iii., Abridged ed., p. 571.

† With regard to what Pliny has written, Dr. Martin Lister remarks (*Phil. Trans.*, vol. iii., Abridgement, p. 572):—"There is but one place that I know of which may give us any sight into the inquiry concerning our tools, and that is in Pliny, lib. 34, c. 14, when speaking of iron, he says, '*Fornacum maxima differentia est; in iis equidem nucleus ferri excoquitur ad indurandum aliter alioque modo ad densandas incudes malleorumve rostra.*' From this passage it should seem that the ancients had one way to make steel, and another way to harden or temper their tools, particularly such as picks and anvils. It is also plain, that 'nucleus ferri' was melted down in both. Again, the difference was in the furnaces—that is, in the manner of ordering the iron to be made into steel, or for the extraordinary hardening of the heads and tips of tools, and

half of the first century of our era—were well versed in the practice of steel-making by the same process; and, seeing that he held the office of Spanish Procurator under Vespasian, it is at least very probable that his observations were made on the mode of manufacture established by the Romans in that country, wherein Toledo has been a famed seat for the production of swords of unrivalled temper and elasticity, from the time of the Romans until now—where, indeed (I believe I am correct in so saying, on the authority of Mr. Cockburn Muir, C.E., who has visited the place), at the present time, the process by which their steel is made, the Spaniards keep secret—a place, indeed, well worthy of mention in these inquiries, seeing that the Roman army was provided with steel swords, many of them made there, with which they conquered the greater part of the then known world, including, too, these British Isles of ours, under Cæsar, B.C. 55.

When the archæology of India and China shall have been penetrated to its depth, we may hope to arrive at something more definite

not in the matter of which they were made, for both were done by boiling them in molten iron. It cannot be doubted but by 'nucleus ferri' must be meant well-purged iron; the same which Aristotle calls ὑιῤζασμένος σιδηρος; for why else should he tell us that wrought iron itself may be made liquid, so as to harden again—that is, to cast again into sow metal—if it was not to explain to us the manner of making steel, which they did probably after the precept above delivered—that is, not only boil the iron in its own sow metal, or liquid iron, but hammer it also, and after that quench it in cold water.

"As to the steeling of their tools, they boiled them in sow metal, to such a degree of hardness or temper as was requisite, and did not afterwards hammer them. And this seems to be implied in the phrase "densare;" for although it be generally said that iron is purged and refined for the making of steel, yet, according to the last and truest process, the matter is plainly otherwise; for iron this way made into steel, becomes a kind of electrum,* and is filled with an exceeding brittle and hard body† of its own nature; iron being spongy and not close; for which purpose, therefore, the word "densare" is by Pliny aptly and elegantly used. And this way was used when the strongest temper and hardness was required, as for picks and anvils. For which there might be several reasons given; as first, that it is easier to work iron than steel, into any figure, that being far softer and more ductile and loose: again, it is certain, that iron by ignition is spoiled or corrupted, so that the oftener it is purged, though it were steel, it would the more relent. Whence the ancients, knowing well, that in making their tools of steel, they must considerably loosen it, and abate of their temper; they therefore first shaped them, and then gave them a strong body of steel and temper together, and so had nothing else to do, but to finish them on the grindstone and hone, to set the point or edge."

* 'That is to say, a mixed metal or alloy.'—St. J. V. D.

† 'Carbon, I presume, is intended.'—St J. V. D.

in respect of the practice in very ancient times, of the art we are considering; until that is done much ignorance thereon is inevitable; and in view of what little knowledge we do even now possess from the sparse nature of the information to our hand, even concerning comparatively late periods, we are led to hope for a great deal when the completer sources of testimony are bared to our vision.

A brief review of what we already have ascertained will be sufficient to convince the most sceptical how ignorant we are of the stages through which metallurgy has passed in the East; and in regard to this state of things Mr. Mallet has so well written, that I cannot do better than quote his *ipsissima verba*:—"It is strange that of the iron metallurgy of Asia, the mother-land of all the arts; of India, the country where, more than twenty centuries ago, King Porus presented to Alexander the Great *a packa*, or wrought bar of Damascene steel, just as Homer's Achilles offered for a prize at the funeral games of Patroclus, a like valued mass, whence the Greeks obtained the like 'wonder-working sword-blades' (θαυμασια ξιφη); where steel dies were employed for coinage when our own ancestors* were naked savages; of China, whence *cast-iron* hollow vessels now reach us of a combined magnitude and thinness that we have not yet been able to imitate, or even to imagine the process by which they have been cast; and razor steel, said to surpass all European steel in temper and durability of edge, less is known than of the iron working of any other part of the civilized world; yet such is the fact.

"Central Asia, and even China, as yet, are nearly *terræ incognitæ;* but India, south of the Himalayas, has been more or less open to European observation for centuries, and has been in our own power for more than one. How little is known of the working of iron in China may be gathered from the bald and imperfect account of it given (pp. 52–58) by M. M. Stanislas Julien, *Memb. Inst.*, and Paul Champion, 'Preparateur au Conserv. des Arts et Metiers,' in their work on the *Technical Arts of the Chinese Empire*, published in Paris in 1869; the first-named author having a reputation as one of the first Chinese scholars in Europe, and the second being a competent metallurgist and technologist. Yet China possesses the knowledge of working in *fluid cast-iron*, as well as in wrought iron and steel—the first being at the present day, at least, commonly reported to be unknown in India, where wrought iron is made direct from the ore, and steel also."

* I presume Mr. Mallet means "the ancient Britons," who were not *our ancestors*.—St. J. V. D.

Judging from what we gather among the monuments of India, the working in iron in very large masses was carried on there many centuries since; for, at the mosque of Kuttub Shaw,* in or near Delhi, is standing at this day an iron column,† which is described as follows by Colonel A. Cunningham in the "Archæological Survey Report to the Government of India for 1861-62:"—

Art. 65. "The Delhi pillar is a solid shaft of mixed metal, upwards

* There is much confusion in the different accounts I have met with as to the precise building in which the iron column is situated. The column itself is usually called "Kutub Minar;" yet, according to the account in the *Asiatic Researches* (vol. iv., p. 727) by Ensign J. T. Blunt, the "Kuttub Minar" is a stone building, between 240 and 250 feet high, built by the Emperor Shaw Jehan in the present city of Delhi; and it appears from this account that this minar—minaret—was erected at the expense of Kuttub Shaw, whose tomb is situated a few hundred yards westward of the Minar. This Kuttub Shaw came to the throne of Delhi in the Mussulman year 602, corresponding with the Christian Era 1205; but the iron column itself is vastly older than this stone building (as shown in the text). It is specially desirable that the confusion which exists on this point should be cleared up; and, by way of anticipating what is probably the fact, we may presume that the column, pillar or Lâht, referred to, is within the precincts of the enormous, never-completed, and now unfinished mosque, named after the name of the founder of the aforesaid stone minar.

† I am much indebted to Col. H. Yule for a sketch of this remarkable Lâht, from which the accompanying plate has been drawn, and also for the valuable references to the literature bearing upon it, contained in the following letter:—

"Giardino Inglese, Palermo,
October 9, 1871.

"My Dear Sir,

"I have not been able hitherto to send the promised communication about the Iron Lâht or Pillar at Delhi.

"There is an account of it in Col. Cunningham's 'Report of the Archæological Survey of India,' for the season 1862-63, which forms a supplementary number to the *Journal of the Asiatic Society of Bengal* for 1864. But Col. Cunningham, though describing it under its popular name of 'the Iron Pillar,' calls it a solid shaft of *mixed metal*, about 50 feet in length, and upwards of 16 inches in diameter. The total height above ground is 22 feet, but it has been *ascertained* to extend at least 26 feet below ground, and the excavation did not reach the foundation, nor was the pillar loosened thereby. Hence Cunningham supposes it may really be not less than 60 *feet* in length. There is an inscription on it ascribing its erection to an otherwise unknown Raja Dhaoa. The character of the letters is considered, both by Prinsep and Cunningham, to refer it to the third or fourth century of our era. It also bears later inscriptions.

"The translation of the inscription will be found in Thomas's edition of Prinsep's *Essays on Indian Antiquities* (1858), vol. i., p. 319-321. No doubt is there expressed as to its material. Prinsep calls it 'a monument of skill in

of 16 inches in diameter and about 50 feet in length. It is true that there are flaws in many parts, which show that the casting is imperfect; but when we consider the extreme difficulty of manufacturing a pillar of such vast dimensions, our wonder will not be diminished by knowing that the casting of the bar is defective. The total height of the pillar above ground is 22 feet, but the smooth shaft is only 15 feet, the capital being 3½ feet, and the rough part of the shaft below also 3½ feet (see drawing). But *its depth under ground is considerably greater** than its height above ground, as a recent excavation was carried down to 26 feet without reaching the foundation on which the pillar rests. The whole length of the iron pillar is therefore upwards of 48 feet, but how much more is not known, although it must be considerable, as the pillar is said not to have been loosened by the excavation. I think, therefore, it is highly probable that the whole length is not less than 60 feet. The lower diameter of the shaft is 16·4 inches, and the upper diameter is 12·05 inches, the diminution being ·29 of an inch per foot. The pillar contains about 80 cubic feet of metal, and weighs upwards of 17 tons." With regard, then, to the constitution of the metal of which this remarkable shaft is composed, Colonel Cunningham

forging iron, for the pillar is a well wrought circular shaft of iron of considerable magnitude.'

"Cunningham again speaks as if it were *cast*. It is a pity the thing is left so obscure. I have seen the pillar twice or thrice, and it certainly seemed to me a shaft of wrought iron to all outward appearance. But, query, whether if it were really iron the surface could have remained perfect as it is—polished and bearing the superficial inscriptions—for 1600 years?

"It is mentioned by the Arab traveller, Ibn Batuta, who was at Delhi circa 1340. He says (*Voyages d'Ibn Bathoutah*, Paris, 1855, vol. iii., p. 150), 'In the middle of the mosque you see an enormous pillar, made of some unknown metal. One of the learned Indians told me that it was what is called *Hâft-âsh—i.e.*, "Seven-metals," being compounded of so many different metals. The pillar has been polished on a surface about as long as one's forefinger, and this part shines brilliantly. Iron leaves no mark upon it. Its length is 30 cubits: we encircled it with the cloth of a turban, and found the girth so measured to be 8 cubits.' The Arab's *girth* is greatly exaggerated; so probably his *length* is also an exaggeration of the height above ground, and bears no reference to the actual entire length, which we see to be more than 30 cubits.

"The Pillar is mentioned by Ferguson, *Hist. of Architecture*, vol. ii., p. 460; but he is curiously inconsistent in calling it 'wrought iron,' and then 'a single casting.'

"Yours very faithfully,

"H. YULE."

* These italics are mine.—St. J. V. D.

erroneously describes it as "cast metal;" and as it does not appear on the face of his report that he personally examined the shaft itself, but probably relied upon what was conveyed to him by others, guided possibly to some extent by the very mystifying account given of it by Mr. James Fergusson in his *History of Architecture*, vol. ii., p. 460, where the column is described in one line as "wrought iron," and in another as "a single casting,"—it is desirable, nay, really important, to show at once what has been done, with the object of settling whether "forging" or "casting" it be. With this object a fragment has been cut from the pillar, and not long since sent to England, a portion of which is said to have passed into Dr. Percy's hands; and this he (Dr. Percy) had heated and drawn out on an anvil—after which he (Dr. Percy) pronounced it to be "forged iron." Strangely divergent, however, from the account given (as already quoted) from Colonel Cunningham's Report is that which has appeared in the last published part of the *Journal of the Iron and Steel Institute*,* where, on the authority of Lieutenant Spratt, R.E., said to have been stationed at Delhi, certain statements are published which are utterly contradictory of those given by Colonel Cunningham. Whether in the main Colonel Cunningham's account or that of Lieutenant Spratt be correct, I am not at present in a position to determine. I am, nevertheless, bound to point out that in dealing with the question, either Lieutenant Spratt or Dr. David Forbes, F.R.S., the writer of the Iron and Steel Institute's Report, are at fault in respect of misrepresenting that which Colonel Cunningham really has written. One or the other of these gentlemen, in the previous issue of the said journal,† represents Colonel Cunningham to have reported, and Mr. Mallet to have repeated, that the above-ground height of the column was 48 feet; whereas in both Colonel Cunningham's report ‡ and Mr. Mallets' account,§ that is the total height of both the above-ground portion and that below-ground, to the depth reported to have been excavated; but now, according to Lieutenant Spratt, the above-ground portion is 24 feet, or 2 feet higher than that portion is reported by Colonel Cunningham, whilst in regard to the below-ground portion, it is not only very short indeed, but of a complex character contrasting strangely and irreconcilably with the description handed to us by Colonel Cunningham. The

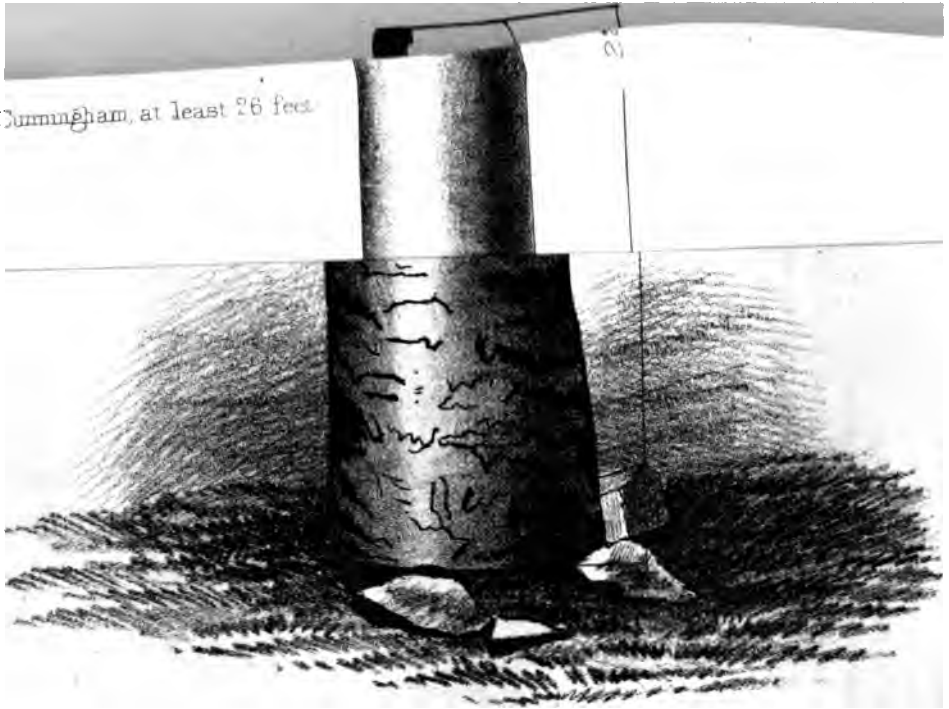
* Vide *Jour. Iron and Steel Institute*, vol. ii., p. 156.

† Vide *Jour. Iron and Steel Institute*, vol. i., p. 151.

‡ *Jour. Asiatic Society of Bengal*, vol. xxxiii., Art. 65, pp. 34, 35.

§ *The Engineer*, vol. xxxii., pp. 403–405.

Colonel Cunningham, at least 26 feet



IRON LÂT AT DELHI

ount to which I now allude reads thus:—"The height of the umn above ground is only 24 feet, and 3 feet below ground ends in a bulb like an onion, which is held in its place by eight ort thick rods of iron, on which it rests, and which at their lower tremity are let into blocks of stone, in which they are secured lead. The iron of which it is made, which appears to have been ginally in blooms of about 50 lbs. weight each, has been amined by Dr. Murray Thomson, of the College at Roorkee, who ind it to be wrought iron, possessing a specific gravity of 6." * With respect to the material of which the pillar, alleged be one and the same, to which both the afore-mentioned reports pertain, is composed, they agree; but it is inconceivable that an icer and Archæological Surveyor to the Government of India of lonel Cunningham's reputation, could be so far led away from ts as to report certainly the most remarkable relic of ancient n metallurgy yet discovered, in the loose and false style he arly has done, if Lieutenant Spratt's account be the correct e. The discrepancies to which I have directed attention certainly ggest the possibility of two different iron pillars existing in or ar the city of Delhi, and that some confusion has resulted on at account; but as we have at present no certain evidence of ore than one such column, and considering the complex, and ally even now-a-days most difficult to produce character of the ging as it is described by Lieutenant Spratt, we must, in the sence of more precise information—in view, too, of the palpable unders in the report of the *Journal of the Iron and Steel Institute* to Colonel Cunningham's statements—and until that information aches us, prefer Colonel Cunningham's account, as being most obably nearer the truth.

Having thus, I trust, judged impartially in the somewhat diffilty of the position in respect to the real form of this monument, e next point we have to deal with is the date of its construction. or this I again refer to Colonel Cunningham's report, Art. 66, *et q.*, in which he concludes for that event the year 319 A.D. †

* *Jour. Iron and Steel Institute*, vol. ii., 1872, p. 156.

† "66. The iron pillar records its own history in a deeply-cut Sanskrit inscripn of six lines on its western face. The inscription has been translated by mes Prinsep, who remarks that 'the pillar is called the arm of fame' (*Kirtti uja*) 'of Raja *Dhava*, and the letters cut upon it are called the typical cuts licted on his enemies by his sword, writing his immortal fame.' It is stated at he subdued a people on the *Sindhu*, named *Vahlikas*, who must be the *hikas* of the Panjâb, and lastly, that he 'obtained with his own arm an divided sovereignty on the earth for a long period.' The above is the whole

With further respect to this column, several writers appear to have confounded it with the stone column, known as Feroze

of the meagre information that can be gathered from this inscription, save the bare fact that the Raja was a worshipper of Vishnu. The date of the inscription is referred by James Prinsep to the third or fourth century after Christ; but Mr. Thomas considers that this is 'too high an antiquity for the style of writing employed on the monument.' I agree, however, with Prinsep, as the characters appear to me to be exactly the same as those of the Gupta inscriptions. I have already suggested the year A.D. 319, which is the initial point of the Balabhi or Gupta era, as an approximate date for Raja *Dhava*, as I think it not improbable that he may have assisted in the downfall of the powerful Gupta dynasty.

"67. According to universal tradition, the iron pillar was erected by *Bilan Deo*, or Anang Pâl, the founder of the Tomara dynasty, who was assured by a learned Brahman that, as the foot of the pillar had been driven so deep into the ground that it rested on the head of *Vâsuki*, king of the serpents, who supports the earth, it was now immovable, and that dominion would remain in his family as long as the pillar stood. But the Raja, doubting the truth of the Brahman's statement, ordered the pillar to be dug up, when the foot of it was found wet with the blood of the serpent king, whose head it had pierced. Regretting his unbelief, the iron pillar was again raised; but owing to the king's former incredulity, every plan now failed in fixing it firmly, and, in spite of all his efforts, it still remained loose (*dhila*) in the ground; and this is said to have been the origin of the name of the ancient city of *Dhili*.

"68. This tradition has been variously reported by different authorities, but the main points are the same in all. Colonel Tod states that the iron pillar is said to be resting on the head of the *Sahes Nâg*, who is the same as *Vâsuki*, the serpent king. A lady traveller, who visited Delhi between 1804 and 1814, heard the tradition in a somewhat different way. A Brahman told the king that if he could place the seat of his government on the head of the snake that supports the world, his kingdom would last for ever. The iron pillar was accordingly driven into the ground on its present site, under the superintendence of the Brahman, who announced that the lucky spot had been found. On hearing this, a courtier, jealous of the Brahman's influence, declared that the pillar was not placed over the serpent's head, but that he could point out the true place, which he had seen in a dream. The pillar was accordingly taken up by the Raja's order, and, agreeably to the Brahman's prediction, the foot of it was found wet with the blood of the serpent's head. This tradition is also imperfectly related in Purchas's *Pilgrims*, on the authority of English travellers who visited India during the reigns of Jahângir and Shahjahan. Purchas states that the *Rase* (Raja) who founded Delhi 'by advice of his magicians, tried the ground by driving an iron stake, which came up bloody, having wounded a snake. This the *Ponde* (Pânde or Pandit), or magician, said was a fortunate sign.' In all these different versions of the erection of the iron pillar, the main points of the story are the same, and the popular belief in this tradition is confirmed by the well-known verse—

Killi to dhilli bhai,
Tomar bhaya mat hin.'
'The pillar became loose by the Tomar's folly.'

Shah's Lâht—described in the *Asiatic Researches*, vol. i., 4to ed., p. 371, as the following will shew:—

"At ancient Delhi," says Bacon, "there is an extraordinary

"69. This tradition is related in a more poetical form by Kharg Rai, who wrote in the reign of Shahjahan. According to him, the Tomar Prince was provided by the sage *Vyâs* with a golden nail, or spike, 25 fingers in length, which he was told to drive into the ground. At a lucky moment, on the 13th day of the waning moon of *Vaisâkh*, in the *Samvat* year 792, or A.D. 736, when the moon was in the mansion of *Abhijit*, the spike was driven into the ground by the Raja. Then said Vyâs to the King—

'*Tum se râj kadi jaëga nahi.*
Yih khunti Vasug ki mâthe gadhi hai.'

'Ne'er will thy kingdom be besped,
The spike hath pierced Vasuki's head.'

"Vyâs had no sooner departed, than the incredulous Raja boldly declared his disbelief in the sage's announcement, when immediately

'*Bilan De khunti ukhârh dekhi,*
Tab lohu se chuchâti nikali.'

'He saw the spike thrown on the ground,
Blood-dropping from the serpent's wound.'

"The sage was recalled by the horrified king, who was directed to drive the stake into the ground a second time. Again he struck, but the spike penetrated only 19 fingers, and remained *loose* in the ground. Once more then the sage addressed the Raja prophetically, 'Like the spike (*killi*) which you have driven, your dynasty will be unstable (*dilli*), and after "nineteen" generations it will be supplanted by the Chohans, and they by the Turkans.' Bilan De then became King of *Dilli*, and with his descendants held the throne for nineteen generations, according to the number of fingers' lengths which the spike had been driven into the ground.

"70. What was the origin of this tradition, and at what time it first obtained currency, may never perhaps be known; but I think we are justified in hazarding a guess that the long reign of the Tomar dynasty must first have led to an opinion of its durability, which would then have been naturally compared with the evident stability with which the iron pillar was fixed in the ground. We have an exactly parallel case in the well-known saying about Rome and the Coliseum—'*Quamdiu stabit Colyseus, stabit et Roma: quando cadet Colyseus, cadet Roma;*' which the verse of Byron has rendered famous:—

'While stands the Coliseum, Rome shall stand;
When falls the Coliseum, Rome shall fall.'

This, indeed, is the oldest form of the Indian tradition that I have been able to trace. When the Muhammadan conqueror first took possession of Delhi, he was informed that the inscription on the iron pillar declared that the Hindu rule would last as long as the pillar remained standing; on hearing which, to show his contempt of the prophecy, the proud victor allowed the pillar to stand. This same story must have been told to Bishop Heber, but he has jumbled it up with his account of Firuz Shah's Pillar.* That the story which he heard

* Journal II., p. 291.

pillar, apparently metallic, but in reality of red sandstone, bearing a silvery bed in it, now called Feroze Shah's Lâht, or walking stick. *Thirty feet of it are above* ground, and buried many feet in the earth. There is one exactly similar to this at the Cootub Minar, and another at Allahabad. All are supposed to be of the same origin, but their history is quite lost in obscurity. They are covered with inscriptions.* Forrest mentions that this pillar at the Cootub Tower is of solid iron,† and that the whole temple is a subject of great doubt and incertitude."

A learned correspondent, too, writes me in respect of this iron column:—"The inscriptions thereon are of very *different dates*. Some, the most ancient in the Nagari letters, others in Sanskrit, all prefaced by the mystical Holy Invocation, O'M. The events engraven involve very great periods,—thus one is decidedly the 'year of Christ 67 (by computation).'"

If this last account, then, has reference to the column described by Colonel Cunningham (which I am bound to say I doubt), it is

must have belonged to the iron pillar is rendered certain by his referring it to the period of 'the conquest of the country by the Musalmâns.' About the same time, also, a similar story was heard by Major Archer,‡ who records that 'as long as the pillar stood, so long would Hindustan flourish.' At a later date, a similar story was repeated to Mrs. Colin Mackenzie,§ who says that the iron pillar bears a Sanskrit inscription, 'the purport of which is, that as long as this pillar stands, the *Râj* or kingdom has not finally departed from the Hindus.' Lastly, Syad Ahmad relates that the pillar was driven into the head of *Vâsuki*, king of the snakes, to make his empire lasting.

"71. If I am right in ascribing the origin of this tradition to a late period in the history of the Tomars, when the long duration of their rule had induced people to compare its stability with that of the iron pillar, I think that the saying may be referred with considerable probability to the prosperous reign of Anang Pâl 2d., whose name is inscribed on the shaft with the date of *Samvat* 1109, or A.D. 1052.

"72. The remaining inscriptions on the iron pillar are numerous, but unimportant. There are two records of the Chohan Raja, *Chatra Sinha*, both dated in S. 1883, or A.D. 1826. They state that the Raja was descended from *Prithivi Raja* in 29 generations, which is quite possible, although the period allowed for each generation is under 23 years. The date of Prithivi Raja is given as S. 1151, or A.D. 1094, which is just 99 years too early, an amount of error which agrees with the false dates in the *Prithi Râj Chohan Râsa* of the Bard *Chand*. There is also another modern Nagari inscription of six lines, dated in S. 1767, or A.D. 1710, of the Bundela Rajas of *Chânderi*. Below this there are two Persian inscriptions, dated in A.H. 1060 and 1061, or A.D. 1651-52, which merely record the names of visitors."

* Bacon's *Studies in Hindostan*, vol. ii., p. 315.

† Forrest's *Jumna and Ganges*.

‡ Journal L., p. 131. § *Ibid.*, 2d edition, p. 46.

unquestionably much older than he concludes. I am of opinion, however, that it relates to the red stone Lâht of Feroze Shah, and therefore has no reference to the iron column. In the midst of such conflicting or rather confused testimony, which is all we yet possess, I conceive it would be unwarrantable to attempt to fix the date of this remarkable work. The utmost, then, at which we can now arrive is, that its antiquity is very considerable—this conclusion being arrived at not only by reason of the absence of any precise generally current information regarding it, but judged of also from the universal testimony throughout the East, both in material as well as literary relics of a very remote and contemporaneously very high intellectuality there prevailing.

As to how this remarkable forging was produced, some have been bold enough to conjecture.* For the present I abstain from offering an opinion; but as a suggestion indicating what may be at least a possible mode, in which large masses of iron were produced in ancient times, I have been referred to the account presented to us by Mr. Alfred Russel Wallace,† of the manner of producing the "sacred krisses" ‡ in the island of Lombock. This was effected at the taking of a census, by the Rajah commanding a needle to be brought to him for each head of the population. These needles were collected by the chiefs in the various villages and towns, and a bundle sent from each to the Rajah, which contained a number of needles corresponding to the numbers of the people; and "when it was quite certain that every village had sent in its bundle, the Rajah divided the needles into twelve equal parts, ordered the best steel-worker in Mataram to bring his forge, and his bellows, and his hammer to the palace, and to make the twelve krisses under the Rajah's eye, and in the sight of all men who chose to see it." Whether then the iron column at Delhi is the result of taking a census after the manner adopted by the Rajah of Lombock, I must leave others to determine; but that it is a monument in commemoration of some grand event, or of some high religious belief and practice, no one, I presume, would attempt to question.

Mr. Mallet, after carefully reviewing the evidence as to whether this huge specimen of ancient iron work be a casting or a forging, says :—

"We are thus obliged to consider that this pillar is *not a casting*, but is a huge forging in native Indian or some other Asiatic-made

* See Appendix B.

† The *Malay Archipelago*. Macmillan & Co., London ed. 1869, p. 284.

‡ The "Kris" is a cutting weapon used by the natives of Lombock.

wrought iron, and if so, the question arises how was it forged? We have no evidence that 'blooms' of more than 90 lbs. or 100 lbs. each were ever made by Indian methods; these would be too small to build up singly into a bar of 16 inches diameter. It is, however, conceivable that such little 'billets' as were procurable from such blooms might be welded up into bars, and these bars made into a faggot, out of which such a bar, by *sufficient means* for bringing it to a welding heat, and for then hammering it, might be welded into a cylindrical bar such as that of this iron pillar.

"Now, the limit to the size of a faggot that can be welded with given means of *heating it*, is found to be when the mass is so great in proportion to the power of the furnace that the exterior of the mass, where the heat is being applied, oxidates and melts away, owing to the slowness of heating, and hence long continuance of exposure to the heat, as fast as piece after piece is laid on to make up for the waste. This limit has been reached before now even in our best reverberatory forge furnaces; it actually was touched upon at Liverpool, in forging the Mersey Company's great 13-inch gun. Unless, therefore, the iron workers of India between the third and fourth century, A.D., possessed air furnaces and lofty stalks, or blowing apparatus of some sort upon a scale now unknown, and, indeed, not conceivable in any form of native apparatus, we may confidently affirm that no faggot to form a welded bar of 16 inches diameter could have been by any possibility brought to the welding heat at all, or without such waste as to prevent its ever being forged.

"If we pass from the heating of such a bar to the forging of it, our difficulties are still greater. The limit in size of *hand forged* work in Europe was about reached in the production in days gone by of the heaviest 'best bower' anchor of a ship of the line. The largest section of the anchor shank when welded to the arms was about 8 inches or perhaps 9 inches across, and the welding was effected by the blows of twenty-four 'strikers' trained to strike in time, and swinging 14 lb. to 18 lb. sledges. The shower of blows dealt for some minutes' spell, upon the mass of iron of this large section produced a very insignificant effect, so that both the faggoting and the welding of such anchors were often very defective, and the strikers having to stand close in a ring, within the short distance for swinging the sledge from the glowing iron, were greatly scorched by its radiated heat, and some with fine skins were unfitted for the work. Hereabouts, then, the limit to hand forging was reached, both as to the power of the hand sledge to act

upon the mass of iron, and as respected the power of the men to endure the heat radiated from the glowing iron at the short distance from it, limited by the length of the handle of a sledge when swinging. Now the section of the shank of a 'best bower' of 8 inches or 9 inches diameter is to that of the Delhi iron pillar about as 64 to 201, or the latter would radiate from its heated extremity more than thrice as much heat, and an equal length more than thrice as great a mass to be dealt with by the sledge hammer, as in the case of the anchor. We may, therefore, affirm that even in European hands a bar of wrought iron of 16 inches diameter could not be welded up by hand labour with the sledge. The latter would produce no adequate impression—least of all in the comparatively feeble hands of Asiatics—and human skin and muscles could not withstand at 5 feet or 6 feet off, the intolerable glare and scorching of such a mass heated to the welding point. How then was this Delhi pillar forged in India, even assuming that some means for heating it existed? Forging by power in some form, of course, suggests itself, but upon what source of power can we even speculate? Human muscles, and the 'bullock walk' by which the water skins, or 'bheesties,' are drawn up from the wells or tanks appear the only present sources of power in India. The water wheel, or *noria*, for *raising* water by the application of such animal power is common; but the production of power by the *descent* of water on a wheel seems never to have been known in India, where, indeed, except in the hill districts, no 'falls' for water-power exist. The windmill, though said to have been known in Persia from some very remote period, has never been seen in India, and it need scarcely be said steam-power is out of the question.

"It is barely imaginable that some form of falling tup hammer raised by men acting on ropes, after the manner of the old ringing engine for pile driving, may have been employed, or some rude form of tup or tilt hammer moved by bullocks acting on a walking wheel; and it is for Indian archæologists to discover if there be any records or traditions of such appliances, without which the methods by which this huge pillar was forged must remain inexplicable. The pillar itself stands before us, so far, a metallurgic enigma; if it stood alone, and were this great ancient forging in wrought iron alone known to exist in India, we might pass it by, content to suppose it too isolated an instance on which to found any conclusions as to the iron metallurgy of that country in former ages; but, although little noticed, and apparently quite unknown to our

European writers on iron metallurgy, this pillar does not stand alone.

"Not to lay any stress on the probable existence in India of *other* iron pillars, as affirmed to the writer by an accurate Indian officer well acquainted with the country, the following facts are recorded by Mr. James Fergusson, in his *Illustrations of Ancient Architecture in Hindostan*, fol., London, 1848, p. 28, and Plate III.

"In the temple of Kanaruc, or Black Pagoda, in the Madras Presidency, 'the walls of the *mantapa* or porch (which is about 60 feet square inside) are about 10 feet in thickness, and the depth of the doorways is, consequently, 20 feet, and their lintels are supported by *large iron beams of about* 1 *foot section*, laid across from side to side. The roof is formed after the usual bracket fashion of the Hindus, each course projecting beyond the other, so as to give (from the inside) the appearance of inverted stairs. . . . At about half the height, where its dimensions narrow to about 20 feet, a false roof has been thrown across, the remains of which now lie heaped up as they fell on the floor of the apartment. Among them may still be remarked *several beams of wrought iron about* 21 *feet in length and* 8 *inches section*, and a great many blocks of stone, 15 feet and 16 feet long (and they were probably broken in their fall), and of a section of 6 feet by 2 feet or 3 feet.' Here, then, we have employed as mere building material wrought-iron bars of 8 inches square and 21 feet long. Mr. Fergusson views this temple as having been built between A.D. 1236 and 1241.

"In another temple examined and described by Mr. Fergusson—that of Mahavellipore, standing alone on a solitary rock of granite projecting into the very surf on the coast, near Madras, and the date of which he refers to the tenth or eleventh century, A.D., but deems may be as late as the thirteenth or fourteenth century, and described in his great work as above, p. 57, and Plate XVIII.—he informs me there are empty sockets for beams, like those just noticed; the beams, he justly argues, must have been of iron, as the sockets show a scantling, which would have been, if in timber, perfectly useless under the load carried.

"Fig. 3 shows, in mere diagram section, the general construction of these *Vimana's* and the position of the iron beams referred to in both the above cases. It is highly probable that at Kanaruc the ceiling beams thrust themselves out of their sockets by alternate expansion and contraction, and so brought down the 'false roof,' as the filling above is called; but the place affords a bad foundation, and is said to be subject to earthquakes. However, our business

with these iron beams is simply as metallurgic monuments. Here, then, we have the fact that at Delhi, in the north, and at Madras, in the far south of India, massive forgings exist, such as all Asia, so far as we know, could not produce at the present day, and of a size rivalling those upon which Europe to-day prides itself. The earliest of these dates from the third or fourth century, and the latest from the eleventh to the fourteenth centuries of our era. With such an interval in time as 900 or 1,000 years, and such a diffusion in space as from north to south of India, it seems impossible not to conclude that the evidence of these monuments attests the existence in India for that long period of *a great iron manu-*

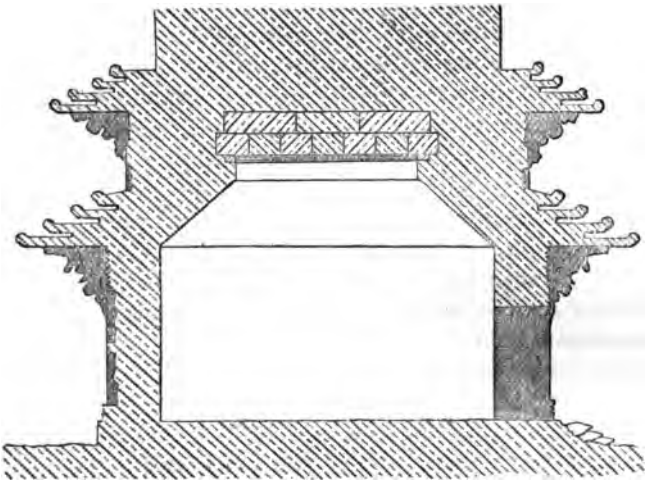


Fig. 3.

facture, well established, and with a relative cheapness and certainty of product that admitted of the use of iron as a material for public monuments and as a building material in sacred edifices, and that this manufacture was extinct, and the art and methods lost, long before any modern European occupation of India. So that far from Indian iron working having been the same feeble thing we see it to-day from time immemorial, it was once a great and flourishing craft, and extended over parts of the entire Indian peninsula.

"Nothing heretofore brought to light in the history of metallurgy seems more striking, to the reason as well as the imagination, than this fact: that from the remote time when Hengist was ruling in

Kent, and Cerdic landing to plunder our barbarous ancestors in Sussex, down to that of our Third Henry, while all Europe was in the worst darkness and confusion of the Middle Ages—when the largest and best forging producible in Christendom was an axe or a sword blade—these ancient peoples of India, the forerunners of those now so enfeebled and degraded, possessed a great iron manufacture, whose products Europe even half a century ago could not have equalled.

"Yet these conclusions rest on no *new* facts, but on the colligation of old ones, by the light of practical knowledge. Indian archæologists and writers have long known of the existence of these iron monuments of an ancient and lost art in India, but their importance has, the writer believes, not before been recognized as bearing on ancient oriental metallurgy. The reason of this is that those who have examined the monuments of India, however scholarly and able in many ways, have not been metallurgists, and have had no practical knowledge of iron working. The ancient, and, indeed, the existing technology at large of India—still more of Asia at large remains almost unexplored and undescribed, and whenever it shall be examined, analysed, and described by really competent men—and such have never yet been commissioned with the task—results even more strange, and perhaps of more importance, historical and practical, than these deducible from the Delhi iron pillar, will, no doubt, come to light.

"Since the foregoing was in type a notice has appeared in *Les Mondes* (tome xxvi., Dec. 1871), by M. Sévoz, an engineer of mines, resident in Japan, of the iron-working districts of that country, which may throw some light on the conjectural modes by which these great forgings may have been effected by human power in those remote ages in India. In reference to the mode of iron-working in the mining province of Ykouno, M. Sévoz says:—"The treatment employed is a sort of imperfect Catalan method. . . . but what distinguishes the Japanese method from that of (Depart.) Ariége is that they treat at once 16,000 kilos. of ore, and produce an enormously long pig of 1,300 kilos., which is broken up under a huge hammer, constructed after the style of a pile-driving ram, to which motion is given by a walking wheel of 11·5 metres diameter, acted upon (montée) by men, one can see that potent blows may thus be given, but their frequency and regularity do not seem such as thus to admit of a forging being produced, even if the means for heating a mass as huge as those referred to were capable of being guessed at."

With respect to the evidence which the literature of the ancient world has to offer in proof of the extreme antiquity of the use of iron, that evidence is considerable and wide-spread, and as this region of our research has already been so extensively explored by the learned and cultivated Goguet, I shall content myself with now briefly running through what he has collected. He says, "The invention of the hammer, anvil, and the pincers was placed by the ancients in the remotest ages. The Egyptians attributed these inventions to Vulcan,* others gave this honour to Cyrinas† the father of Adonis, which makes them no less ancient. In a word, the hammer and anvil are mentioned in the book of Job."

"The art of forging metals, without all doubt, was known in Egypt, and some parts of Asia, in the most distant times. We see arms of metal in use in Palestine, a few ages after the flood. Moses says that Abraham drew his sabre to sacrifice his son Isaac. The ancient patriarchs shearing their sheep is a further proof that the fabricating of metals was then well understood. They knew even then how to execute works in gold and silver, which require delicacy and exactness. In a word, we see everything relating to metals, the places where they are formed, the manner in which they are wrought, &c., exactly described in the book of Job."

Arguing as a pure rationalist, and as one unacquainted with the practice of working in metals, M. Goguet is at this point led off into that favourite but mistaken theory which has taken firm hold upon the minds of those who are versed in the *literature only* of metallurgy, and he proceeds to argue that, *ex necessitate*, mankind would begin to work with "such metals as were most easily procured and fabricated;" he cites gold, silver, and copper as examples, because "considerable masses of these metals were sometimes found pure and without mixture," and "that they were easily melted and refined. For these reasons," he says, "gold, silver, and copper were the first metals that were wrought," and he adds, "*But the finding and working of iron must have been a very late and difficult discovery. Without doubt it was the last of all the metals mankind were acquainted with or knew how to use.*" . . . .

* The worship of Vulcan was well established, particularly in Egypt, at Athens and Rome. His forges were supposed to be under Mount Ætna in Sicily. Vulcan was generally represented as covered with perspiration, blowing with his arm the fires of his forges, and sometimes holding a hammer raised in the air ready to strike; while with the other hand he turned with pincers a thunderbolt on an anvil. (Note added by St. J. V. D.)

† Pliny, L. 7, section 57.

"In reality, the greatest obstacle to the use of iron was the difficulty of making it. Iron, of all metals, *is the most difficult to bring into fusion.*"

In these very words and some others which occur in M. Goguet, Book II. chap. iv., entitled, *Of the discovery and working of Metals*, we find nothing less than the identical doctrine which has been revived in these latter-days, namely, that of the successive use by man of stone, bronze, and iron, which has received the support of so many—and noted persons too—propounded in France more than a century ago. That that doctrine is no longer tenable I have in my former paper shown—and as to the difficulty set up by these who argue from mere book-knowledge, I have already pointed out from the facts of metallurgic science—that *fusion is not essential to the reduction of iron into the metallic state;* that, in fact, to reduce it is one of the simplest and most readily occurring results in the whole range of metallurgic art—that, in fact, the most widespread practice of reducing iron is effected without fusion: and as if to make this difficulty set up concerning the necessity for *fusing* iron in order to work with and convert it into tools, weapons, or other instruments, appear still greater, M. Goguet, as if following after or leading on the *literateurs*, adds, "Besides, one single *melting* is sufficient to render gold, silver, or copper, ductile and malleable. It is not so with iron. A piece of this metal once *melted* is as untractable as ever, and not more ductile than a flint. In this state it is still hard and brittle, and cannot bear the hammer either cold or hot. The file, the chisel, and the graver, can make no impression upon it. In order to forge iron, therefore, it was necessary to find out the art of softening it, and rendering it ductile. To do this it must be *melted* a second time, then beat with very heavy hammers; this mass must be put into the furnace, and heated till it is upon the point of fusion, then put again in this state under the hammer: this operation must be repeated several times; at last this hard and brittle mass, by frequent heating and hammering, becomes forgeable." And yet, M. Goguet, with all this plausible argument against an extreme ancient use of iron, in the end finds it insufficient, directly contrary to the evidence with which he has dealt, so that he concludes his researches in the following words:—

"In maintaining, however, that originally copper supplied the place of iron, *I do not pretend to affirm that this last metal was altogether unknown in the ages we are now upon. Several testimonies give us reason to believe that some nations knew the art of working*

iron in very ancient times. There was a tradition among the Egyptians[1] that Vulcan had taught them to forge arms of iron. The Phœnicians also ranked among their most ancient heroes two brothers, who were supposed to have discovered iron, and the manner of working it.[2] The Cretans, as Diodorus relates, placed both the discovery of iron and the art of working it in the most remote periods of their history.[3] The inhabitants of Mount Ida pretended to have learned the art of working this metal from the mother of the gods.[4] In a word, Prometheus in Æschylus boasts of having taught mankind the fabrication of all metals.[5] Some authors ascribe the discovery of the art of working iron to the Cyclops,[6] and some to the Chalybes,[7] a very ancient people, renowned for their skill in working this metal.[8] The Chalybes inhabited the south coasts of the Euxine Sea, between Colchis and Paphlagonia.[9] Clemens Alexandrinus pretends that the art of making iron malleable was found out by the Noropes.[10] This nation was situated in Pannonia, along the banks of the Danube, between Noricum and Mœsia. Without entering into any discussion of these traditions, which are all liable to great difficulties and contradictions; it appears from the book of Job, that the art of working iron was known in some countries in the ages we are now speaking of.[11] The books of Moses also furnish us with very strong proof of the antiquity of this discovery. From the manner in which this legislator speaks of iron, it appears that metal must have been long in use in Egypt before his time. He celebrates the great hardness of it;[12] takes notice that the bedstead of Og, King of

[1] Chron. Pascal, p. 45, C.; Cedren., fol. 19, D. There is a manifest contradiction in Cedrenus. For after he has said that Vulcan taught the Egyptians to forge arms of iron, he adds, that having obtained pincers, by his prayers, from heaven, he used them in working copper.

[2] Sanchon, apud Euseb., p. 35, C. [3] L. 5, p. 381.

[4] Sophocl. apud Strab., l. 10, p. 725; Diod. l. 17, p. 726; Auctor Phoronid. apud. Schol. Apollon., ad. l. 1, v. 1129; Strabo, l. 10, p. 726; l. 14, p. 966.

[5] In Prometh, *vincto*, v. 501.

[6] Plin. l. 7, sect. 57, p. 414.

[7] Ammian, Marcell., l. 22, C. 8, p. 312; Schol. Apollon., ad. l. 2, v. 375; Tzetzes, chil. 10, v. 338.

[8] Æschyl. in Prometh, *vincto*, v. 113; Virg. Georg. l. i., v. 58.

[9] See Martiniere's Dictionary, *voce* Chalybes, art. 3, and Dunville's map of the retreat of the ten thousand.

[10] Strom. l. 1, p. 365.

[11] Chap. xix. 24; xx. 24; xxviii. 2; xl. 13; xli. 18.

[12] Lev. xxvi. 19; Deut. xxviii. 23 and 48.

Bashan, was of iron;[1] he speaks of mines of iron;[2] he compares the severity of the servitude of the Israelites in Egypt to the heat of a furnace for melting that metal.[3] But what is most worthy of our attention is that they then made swords,[4] knives,[5] axes,[6] and tools for cutting stones,[7] of iron. To make the blades of swords and knives they must have known the arts of tempering and turning iron into steel. These facts seem to me sufficient to prove that the discovery of iron, and the arts of working it, were extremely ancient in Egypt and Palestine."

After all then that has been advanced, I unhesitatingly ask, are not the proofs both material and literary which have been now, I believe, for the first time collected together, corroborative of each other, and do they not demonstrate that in the very earliest ages we can fathom iron was used and well-known? It may have been, nay, relying on the Hebrew records, it is by them thoroughly attested that very many, possibly all, the metals were used simultaneously in the very earliest ages. In the oldest Hebrew books we find them prominently mentioned. That numerous other proofs are in existence I have little doubt, but I must leave their collection to others having more leisure than what has fallen to my lot; and conscious as I am of the defects of this paper, yet I do hope to have at least demonstrated in respect of one particular material, how pre-eminently unsafe it is in drawing conclusions as to the state of the arts in remote past ages, to assume as universal that which appears at most as probable only, from the material relics met with in some countries only. I am further bound to say, that those who have so concluded in modern times seem to disregard altogether what ancient authors have recorded. Thinking only to satisfy some favourite theory, such procedure is not only blinding at the first, but as its sole result not only never can evolve the truth, but must inevitably tend further and further from that sublime region.

In concluding this paper, then, I feel I cannot express what I wish finally to convey, as the issue to which the investigation points, more fitly than in the words of my cultivated and talented correspondent, Mr. Robert Mallet, F.R.S., who, in writing to me in June, 1871, on the subject of ancient iron work, says, it is a subject "on which the usual, if not orthodox, views of archæologists generally seem to me as very superficial. They all seem to forget that, at all times—as now

[1] Deut. iii. 2. [2] *Ibid.* viii. 9. [3] *Ibid.* iv. 20.
[4] Num. xxxv. 16. [5] Lev. i. 17. [6] Deut. xix. 5.
[7] Deut. xxvii. 5.

civilization was simultaneously at various stages, in different parts of the world—when flints were the best tools of N. W. Europe, iron and steel may have been those of peoples far to the East. As in palæontology, you can fix no horizon common to all the world at any time;" or as Dr. Percy, one of the foremost living authorities on the metallurgy of iron, in writing me from the House of Commons, under date July 5th, 1871, quite as appropriately expresses himself,—"I become more and more confirmed in the opinion that archæologists have been generally mistaken concerning the so-called 'Iron Age.' I am collecting further information on the subject from time to time, and as yet have met with nothing of late in opposition to the opinion above-mentioned."

APPENDIX A.

In speaking of the great improbability that the iron of the ancients could have been of extra-terrestrial origin (meteoric iron), Dr. Thorpe remarked, in the discussion which followed the reading of the paper, that,—"Even supposing such iron was formerly more abundant than it now is, we have direct evidence to show that it would be utterly unfit for the purposes of iron manufacture. The experiment of forging such iron has been more than once tried, but without success. The speaker instanced the remarkable attempts recently made by Professor Mallet of Virginia, U.S.A., the results of whose trials were exhibited before the meeting of the Chemical Section of the British Association at Brighton, in September last. These forgings were of a very imperfect character, and so brittle that the greatest care was necessary in handling them to avoid fracturing them. All meteorites contain more or less nickel, cobalt, and phosphorus: these elements appear to exist in union in the meteorite, giving rise to a compound which has received the name of Schreibersite. The peculiar crystalline configurations which some meteorites exhibit, known as Widmanstädt's figures, are in all probability due to the dissemination of this compound through their mass. It is well known that any appreciable quantity of phosphates, and that even traces of nickel and cobalt materially impair the useful properties of iron. Indeed, if the amounts of these substances at all approach those present in the majority of meteorites, the malleability and ductibility of the metal are destroyed. There is little doubt that the impossibility of forging meteoric iron has arisen from the presence of these substances. We must assume, therefore, that the chemical and metallurgical knowledge of the ancients was far more profound than ours, before we can believe that they possessed the secret of working a character of metal that would be utterly useless to-day." To this Professor Thorpe has added in a note since the reading of the paper,—"In conversation the other day with Professor Bischof, he mentioned to me that some time ago Professor Noeggerath, of Bonn, attempted to forge a meteorite, but failed completely."

APPENDIX B.*

THE MANUFACTURE OF LARGE MASSES OF IRON IN INDIA.

"MR. MALLET'S article in *The Engineer* of the 15th ult., on the very singular iron column within the mosque of the Kutab, near Delhi, is one which cannot fail to be particularly interesting to all students of the history of iron metallurgy, and is certainly in great measure exhaustive and complete. Mr. Mallet, however, whilst coming to the conclusion that this monument is of malleable metal, seems yet inclined to suggest the possibility that at some distant date the iron workers of India may have had a knowledge of iron in its liquid form, which at present they do not seem to possess, and of which knowledge history affords us no record. Mr. Mallet's great difficulty—and at first sight there can be no question that it is apparently an insurmountable one—is that, assuming the column to be of wrought iron, of forging such a mass of metal at a welding heat by the mere manual power within reach of Indian iron workers at the supposed date of its manufacture. The experience of many years spent in charge of an iron works in Southern India, where cast iron was produced by the European method, but which experience also comprised constant intercourse with the native smiths of the country, and a knowledge of the material they used, and of the method of its production and capabilities in manufacture, may perhaps entitle the present writer to offer what he ventures to believe will be considered by practical men a satisfactory explanation of how such material, labour, and capabilities might have been used to produce the column now under notice.

"In the first place, then, the writer would record his decided opinion that the column of the Kutab is of wrought, or at all events is of malleable iron; for during the whole course of his Indian experience, which included many visits to the native smelting furnaces in the Salem and Malabar Collectorates, together with the constant practice at his own works in the production of *edged*, not *chipping* tools, of the native steel; he never found anything approaching an attempt to *tap* one of these furnaces, nor heard any Indian workman speak of cast iron but as of a material utterly useless to him, and beyond his ken.

"The process of smelting, as pursued in Southern India, is probably sufficiently well known not to require any further description here, than, that in a perpendicular circular furnace about 6 ft. or 8 ft. in height, and of a diameter at its greatest width of about 18 in.—the blast to which is supplied by the alternate inflation and compression of four or six goat skins worked by hand, as in the ordinary smiths' fires of the country—the black magnetic oxide so common in the laterite formation is converted not into cast iron, but rather into a mass somewhat similar to the loup of the Catalan forges, presenting in parts a crystalline and in others a fibrous fracture. The removal of these lumps—"mootees" they are called by the natives of Malabar—or loups, necessitates the breaking open of the whole of that part of the little furnace which corresponds to the tymp and fore hearth of an English blast furnace; and in order to prepare for this the charging at the top is stopped, as is also the blast, and the whole contents allowed gradually, as combustion exhausts itself, to sink down into the hearth, whence, when cool, it is removed. These loups or mootees (the

* From *The Engineer*, vol. xxxiii. pp. 19 and 20.

writer must object to Mr. Mallet's term 'pig,' as applied either to these or any result of the cementation process, as the term certainly conveys, to English ears at least, the idea of cast iron) are generally from 80 lb. to 112 lb. in weight, and it is from the building up of lumps of metal such as these, one upon the other, with such reheating and hammering as may have been found necessary to effect cohesion, that the writer conceives the Kutab column to have been produced. He cannot think that there is anything impossible in such a mode of proceeding, nor anything in the actual working of the material—which is of a most malleable nature, and weldable at a very low comparative heat—which the native smiths are unequal to performing. Fifteen inches diameter is certainly a very, very large bar; but it should be recollected that in the process just suggested it would only be the surface of each successive mootee (previously, of course, heated and hammered to the proper section) which would require to be at welding temperature, and that such a temperature for such surfaces might readily be produced in good charcoal fires without much injury to the iron so treated. The writer has himself seen shafting of between 6 in. and 8 in. diameter heated in open fires composed of charcoal and 'bratties' (sun-dried cow-dung), and welded to good joints by native smiths in the Madras Presidency. Conceiving, then, that the column may have been thus built up—and of course this supposition is directly opposite to the idea that it might have been composed of longitudinal bars welded together—we find the capital left to be accounted for; and the very form of this, is one which could readily have been produced by swaging, and finishing with such chipping (but this only to a small extent, the writer believes), as may have been found necessary. It is also to be recollected that the column itself has never, save, of course, in the act of raising it, been submitted to any severe strain, and that its cohesion has never in any way been tried in tension, as is ordinary shafting. Further, the extraordinary amount of quiet perseverance with which the natives of India are endowed, and the illimitable amount of mere manual labour which any great Eastern ruler could bring to bear upon such an object of ambition as the construction of a trophy or monument as this column may be considered, would all go to help us to the conclusion that this huge rod of iron may have been manufactured in such manner, and with such material and appliances as the writer has described. Again, it may be remarked that, even supposing other similar columns to exist, as Mr. Mallet seems to think, yet even this very existence, in so confessedly small a number, proves them to have been quite exceptional productions, and not in any way portions of a systematic manufacture of large iron forgings. It is, too, a point well worthy of notice, that there would seem to be no examples left of what might be described as the intermediate stages of iron-working; *id est*, examples of forgings which, whilst exceeding greatly in size and weight the present ordinary productions of the Indian iron smiths, would yet be of far smaller dimensions in every way than this column of the Kutab; for the large beams mentioned Mr. Mallet can scarcely be classed in this category. The writer is therefore forced to the conclusion that this, and also the similar columns spoken of, must be regarded as purely exceptional productions—types of no manufacture ever extensively or usefully existing in India, and indicating neither the possession of machinery calculated to produce such types in any number, nor even much smaller forgings. Exceptional, however, as they appear to be in every way, he yet ventures to believe he has pointed out the

process by which, in all probability, they were manufactured; and if they can be regarded but as mere monuments of some now nameless ambition, they are yet wonderful examples of that ant-like perseverance and patient industry which in many ways mark the metal workers of India.

"GEORGE M. FRASER."

"P.S.—May not the words 'mixed metal' mean a mixture apparently of wrought and cast iron, which is clearly the characteristic of the crystalline and fibrous fracture of the native loups or mooties; and has the great depth, and consequent weight of the column under ground, been used as a counterpoise in raising it into a perpendicular position?"

XII.—*Volcanic Vapours of Mount Vesuvius.* By MR. J. ANDERSON.

Read before the Chemical Section, 9th December, 1872.

IN the beginning of October last year I visited Italy, accompanied by a few friends. We left Rome for Naples by the morning train. The day was warm, the sky clear, and the air dry and dusty; but grapes were abundant at all the stations along the line, and with these we refreshed ourselves as we proceeded on our journey.

Towards evening, as we approached Naples, we saw two small clouds slowly floating away from a mountain at a considerable distance. This mountain we took for granted to be Vesuvius. One of the clouds issued from the side, the other from the summit.

These clouds, as we approached them, and as seen against the clear blue sky, seemed white, but not dense. We could see no trace of fire until darkness set in, and then only two or three small streaks on the side of the mountain, a considerable distance from the top.

In travelling along by the railway, we noticed that the white vapours issuing from the locomotive disappeared at the distance of a few yards from the funnel, being absorbed by the dry air. The thin white clouds that issued from the side and top of Vesuvius, on the contrary, were persistent, continuing to float for a long time against the dark blue sky as far as the eye could follow them.

From this it may be inferred that the clouds were not steam, or the vapour of water; for if so, they would have been absorbed by the dry hot air which surrounded them, in the same manner as the vapour from the funnel of the locomotive.

Having accomplished about three-fourths of the ascent, we arrived at the foot of the small cone which emitted one of the little white clouds. From its crater the latest flow of lava had been ejected, which took place a few months previous to our visit. Some of this lava was still fluid, flowing slowly under a solidified crust down the slopes of the mountain. On arriving at a precipice it trickled over and became visible, forming the streaks of fire seen from Naples. The small cone which projected from the side of the mountain would be about 50, or at most 60 feet high; its sides were rather steep, particularly towards the summit, and a considerable effort was required to reach the mouth of its crater. On arriving there we found the position anything but comfortable, the wind whirling clouds of dust and vapour about our heads and eyes.

The mouth of the crater would not be more than 8 or 10 feet in diameter, and the boiling lava which emitted the vapours would not be more than 15 or 20 feet from where we stood, while the thin wall of lava on which we had to lean for support, and which was the only thing to prevent us slipping into the crater, was not more than 6 inches thick at the edge. However, it was firm, being formed of the fused lava and scoriæ firmly cemented together. The cloud of vapour generally filled the interior of the crater, and on putting the hand down it felt hot and moist. Occasionally a gust of wind would blow the vapours to one side, and the red glare of the boiling hot lava beneath could be seen. On looking down into the crater the vapours seemed white and somewhat dense; under the rays of the sun they had the granular or vesicular appearance of ordinary steam escaping into the atmosphere. The volume was not large, and just as it escaped from the summit of the cone, it seemed to loose it vaporous appearance and become resolved into a very thin white smoke, the surrounding dry hot air absorbing all the aqueous vapours.

I noticed that the walls of the crater were covered with a loose flocculent covering of a light gray colour. This substance adhered to the sides of the crater, in the same manner as soot to the inside of a chimney, lining them to a depth of several inches. It seemed to me that this deposit was precipitated from the vapour as it ascended from the boiling lava. I gathered a handful of what was deposited close to the mouth of the crater, and on trying to collect some more as far down as I could reach, I found it so hot that I was unable to retain it in my hand. Several cwts. of this substance could be gathered from the sides of the crater.

A portion of this deposit was analyzed by Mr. Michael Cochran

in the laboartory of the Glasgow University and was found to contain per cent.—

Water,	5·4
Silica,	·42
Oxide of Copper,	·79
Peroxide of Iron,	15·43
Alumina,	1·30
Oxide of Manganese,	·07
Lime,	·28
Magnesia,	·32
Potash,	22·04
Soda,	14·10
Chlorine,	4·58
Sulphuric Acid (Anhydride),	36·08
	100·81
	1·03
Less Oxygen equivalent to 4·58 cl.	99·78

In addition to the above substances should be mentioned Lithia, which, by the spectroscope, Dr. Thorpe discovered to be present in sufficient quantity to be capable, he thought, of being estimated.

The remarkable features of this analysis are the large proportions of potash, soda, iron, and sulphuric acid found in the deposit.

The interesting enquiries suggest themselves,—In what form did these substances exist in the molten lava? What gaseous compounds did they form when escaping from the red-hot lava? How would they be affected on coming in contact with the atmosphere?

Professor Phillips says that the composition of lava appears to be pretty uniform in all the periods of Vesuvian activity; and in one of the analysis he gives the composition—

Silica,	48·02
Lime,	10·18
Alumina,	20·78
Perox. and Protoxide of Iron,	7·97
Magnesia,	1·16
Soda,	3·65
Potassa,	7·12
Sulphuric Acid,	trace.

Our guide imbedded a coin in the semi-fluid lava that was flowing down the side of the mountain: on cooling, the coin became fixed in the lava.

One of our party, Mr. Harry J. Smith, who is a member of this Society, made a rough estimate of the potash, soda, and iron contained in the lava that adhered to the coin, and found it to

contain fully as much potash, soda, and iron as that given by Professor Phillips.

The potash and soda being in the state of silicates, or aluminates, are extremely fixed even at very high temperatures; but in the presence of other gases, a portion of the alkalies may be volatilized and separated from the silica and alumina.

A very large quantity of steam is known to escape from the craters of active volcanoes. This steam would leave the lava at a very high temperature, and facilitate the escape of more or less of its volatile substances. It is well known that the presence of a gas in a liquid greatly facilitates the conversion of the liquid into vapour—*e. g.*, water does not under ordinary circumstances boil until it is raised to a temperature of 212° Fahr.; but a cubic foot of air passed through it, at a temperature several degrees below the boiling point, will carry a cubic foot of steam along with it. And if alcohol, vinegar, hydrochloric acid, or other volatile liquid be mixed with water, each is carried off in proportion to its volatility.

Glass is a species of lava, containing a much larger proportion of potash or soda than natural lava. The glass maker has to protect his fused material from the hot gases of the furnace, to prevent them carrying off the potash and soda in the glass as vapours. Again, when a bit of soft glass is kept fused for some time in the flame of a blowpipe, so much of the alkali is carried away by the gases of the flame as to render the glass almost infusible. In the same manner, the steam and other gases passing through the red-hot molten lava may be expected to carry potash, soda, and other volatile substances along with them, these substances being again precipitated from the gases, as they are cooled, on the sides of the crater.

There is some difficulty in forming a satisfactory opinion as to where the water which forms the steam escaping from volcanoes comes from. If we suppose the crust of the earth between the lava and the water of the sea to crack, and the water to rush in at the fissure, we should naturally expect that the moment the water touched the hot lava, it would be converted into steam; and unless the pressure be enormous, the steam formed would force the water back again. Should this action really take place, it seems sufficiently powerful to account, in a great measure, for the sea waves and earthquakes that accompany volcanic eruptions, and may even contribute largely to the eruption itself.

A column of water having the diameter of the vent of Vesuvius,

and half the height of the mountain would contain 30,000,000 to 40,000,000 tons of water. Assuming this vast body of water to be urged from the vent into the sea with all the force that red-hot water forming into steam can impart to it, the rush would be powerful enough to generate a wave having sufficient momentum to carry it so far inland as to submerge cities, and leave the shipping of seaports high and dry on the land.

Those who are in the habit of sailing in some of our river steamers are familiar with the noise that accompanies the escape of steam into cold water, when the vessel stops for passengers at the various piers. Sometimes the thuds caused by the sudden condensation are so strong as to make the whole ship tremble. Only a very small quantity of steam is required to produce each of these thuds, seeing that they are generated at the rate of hundreds, if not thousands per minute. If the end of the pipe that dips into the water were visible, it would be seen that the water rushes in and out with the rapidity of a weaver's shuttle, a shock being produced every time the water is drawn in and driven out. The cold water entering condenses the steam as it advances, until the water next it becomes so hot as to be unable to condense any more. The steam then forces the water out of the pipe, and as soon as the steam again comes in contact with the cold water the same process is repeated, and a series of miniature earthquakes are produced.

Now, in like manner, the steam that forced the column of water out of the vent or fissure would come in contact with the cold water of the sea. The moment it did so it would begin to condense, forming a vacuum into which the sea water would again rush, and, urged on by its own weight and the weight of the atmosphere, it would fly back towards the red-hot lava, and the lava towards it, with a force resembling the meeting of two railway trains multiplied a thousand or a million fold. Should such an event take place, the sudden pressure resulting from the impact of such enormous masses at the point of junction would be sufficiently powerful to make the earth tremble.

It is difficult to conceive of conditions by which the water would force itself in among the fused lava, so as to penetrate so far into the cone as to permit the steam formed to boil up through the lava to the top of the mountain. If some such conditions as exist in the Gifford injector were realized, then the injected water would form into steam and produce all the effects required for an eruption.

There is another way in which a quantity of water may find its way quietly into the lava. Some rocks and minerals, such as chlorite schists, gypsum, zeolites, and others, contain a considerable quantity of water, not as water in a sponge, but in chemical combination, as is the case in soda crystals. Supposing a current of lava to come in contact with such rock, and that the temperature of the lava is sufficiently high to fuse the hydrated rock, the water will either be relieved as a gas, or steam, or be retained in combination. Assuming the melting of the hydrated rock to take place at a considerable depth, notwithstanding the very high temperature, there are several reasons for concluding that the water would fuse with the rock and form part of the liquid lava. First, the pressure may be enormous. Supposing the hydrated rock to be not lower than the base of the mountain, the pressure on it would be equal to 4,000 feet of lava. A block of lava, one foot long, having a cross section of one square inch area, would weigh about a pound, giving a pressure on the square inch of 4,000 pounds, equal to the weight of 266 atmospheres.

The conversion of carbonic acid, hydrochloric acid, ammonia, and other gases into liquids, under high pressure, is well known. The presence of a liquid greatly facilitates condensation—*e. g.*, under the ordinary pressure of the atmosphere, water converts many gases into the liquid form, and the greater the pressure, the greater is the quantity of gas absorbed and liquefied. There are certain facts also, that lead to the conclusion, that very moderate pressures are sufficient to retain many gases in the liquid form, even at high temperatures, when other substances are present,—*e. g.*, when oxide of silver is heated to redness, it loses its oxygen and becomes pure silver; but if the heat be continued until the silver melts, oxygen is reabsorbed from the air until twenty to thirty times the volume of the silver is taken up. When the fused silver is allowed to cool, the oxygen boils out of it before it becomes solid. Here we have the same relation subsisting between fused silver and oxygen as subsists between carbonic acid and water. Before freezing, water parts with its carbonic acid gas in the same way as silver parts with its oxygen. Seeing, then, that silver absorbs and liquefies twenty times its own volume of oxygen under the pressure of the atmosphere, how much would it absorb and liquefy under the assumed pressure of 266 atmospheres?

Iron founders are very much annoyed with the tendency that iron has to absorb gases when fluid-hot, and giving them out again while cooling; and they are put to some trouble to provide for

their escape in such a manner as not to injure their castings. A head of metal is formed on the mould through which the gases escape, and which supplies fused metal to the space occupied by the gases. The head is found to be in a frothy or spongy state, something like a piece of porous slag or lava. These small gaseous bubbles, or cells, are found to weaken the iron very much; and Whitworth's latest improvement in gunnery is the prevention of the formation of these cells, by cooling the metal under a very great pressure. The gases are prevented from expanding, and are retained in chemical combination with the iron. In this manner he obtains a metal strong enough to enable him to make a gun capable of sending a shot or shell seven to eight miles.

But it is not only with fused metals that gases have a tendency to combine at high temperatures. Slag from smelting furnaces absorbs and retains gases while hot, which are given out on cooling. In the act of separating and expanding, the gases impart to the slag all the appearance of cellular lava.

When carbonate of lime, such as chalk, or limestone, is heated under pressure, the carbonic acid remains in combination with the lime, and a fused carbonate of lime is obtained. If the pressure be removed before cooling, most of the carbonic acid assumes the gaseous form and escapes.

When oxide of lead—litharge—is heated until it fuses, it does not give out its oxygen, as silver does, but absorbs more, in the same way as melted silver; and what is more remarkable, when silver or litharge is heated with steam, they combine with its oxygen and leave the hydrogen free. According to Deville, the oxygen is separated from the hydrogen by the heat before it combines with the silver or litharge.

The influence of heat, under certain conditions, in separating chemically combined substances into their elements is well known; while pressure has the opposite effect, of causing chemical combination. In volcanic phenomena we have these two contending influences in great force. The temperatures and pressures are enormous, the two combined being almost beyond the reach of laboratory experiments. Judging, however, from what we know, I think we are justified in coming to the conclusion that at no very considerable depth the water, carbonic acid, and other gases are in combination with the silica, alumina, lime, potash, soda, oxide of iron, and other substances which constitute the lava, and that they form one homogeneous fluid, in which all the ingredients are intimately mixed together.

Such a lava, rising obliquely or perpendicularly up the vent of a cone, will experience a diminution of pressure as it ascends. The more volatile substances will begin to assume the gaseous form, generating a mass of small cells, which will expand a thousand fold as they ascend, converting the lava into a species of froth, from which the gases will escape, on arriving at the crater, like steam out of a boiling-over pot, or the gases out of a soda-water bottle when the cork is removed. The steam and gases so escaping form the white cloud seen to issue from every active volcano. When the water vapour is absorbed by the surrounding air, there remains the thin persistent cloud formed of substances similar to that found in the deposit, but in a sublimed state.

If water form a constituent of the fused lava under pressure in the way I have supposed, it is easy to account for the cells of water found in the crystals of which granite is composed. If lava were permitted to cool slowly under the required pressure, the elements of which it is composed would elect to crystallize into felspar, mica, and quartz, the water separating and forming cells in the crystals.

Should the lava in the volcanic vent be in the cellular or spongy state supposed, it will be very susceptible to variations in the atmospheric pressure. As the pressure increases the cells will become smaller, and as it diminishes they will expand, the body of the lava expanding with them, filling the crater sometimes to overflowing. This seems to be confirmed by observations made on Stromboli in the Lipari Islands, in the Mediterranean, a volcano which has been in moderate activity since the time of Homer. Dr. Wollaston says,—"The inhabitants of Stromboli positively make use of the volcano as a weather glass. They are mostly fishermen, and while engaged in their occupation, at a short distance from the island, have its orifice constantly in view; and I was assured by all whom I questioned on the subject, that its phenomena decidedly participate in the atmospheric changes, increasing in turbulence as the weather thickens, and returning to a state of comparative tranquillity with the serenity of the sky."

In the boiling of the vapours out of the lava, and their condensation on the sides of the crater, there is a process of distillation going on, whereby the more volatile metals and their compounds are separated from the more fixed silica, alumina, and lime. In the continuous operation of volcanic action, it may be asked, to what extent does this distilling, or volatilizing action contribute to the formation of metallic veins? Here we have the means whereby metals, such as gold, silver, copper, zinc, and iron may be separated

from masses of fused lava which may not contain a thousandth part of these metals.

Professor Phillips mentions only a trace of copper as existing in the lava, while Mr. Cochran found about ·8 per cent. of oxide of copper in one deposit, and as much as 2·58 per cent. in another. How far these may fill up cracks and fissures, and contribute to the formation of veins of metals, is a question for the mineralogist and geologist.

The crater of Mount Vultur, in the centre of Italy, has been a lake from the time of Horace, and gaseous exhalations still proceed from it. If the gases and vapours from the lava pass up through the water, the copper, iron, potash, soda, and other substances contained in the vapours will be taken up by the water, and certain chemical actions may take place that will precipitate the oxides of iron, copper, and other metals to the bottom of the lake.

Mr. MICHAEL COCHRAN, M.A., made some remarks upon the analysis referred to in Mr. Anderson's paper. He described one specimen which he had received as a gray amorphous salt, having the appearance of a finely-divided ash. In composition it differed much from ordinary lava, no less than 84·3 per cent. being soluble in water, and 99·58 per cent. soluble in strong HCl. The small quantity of silica present, and the large quantity of sulphuric anhydride, indicated that it had been formed by a process of sublimation. When ignited over the Bunsen flame it lost 10 per cent. of its weight, 5·4 per cent. being moisture, the remainder being sulphuric anhydride, from certain of the sulphates present, and also chloride of sodium. He regarded the following as representing the form in which the substances given in the above analysis were combined:—

Sulphate of potassium	(K_2SO_4)	40·80
,, sodium	(Na_2SO_4)	23·13
,, copper	($CuSO_4$)	1·59
,, aluminium	($Al_{23}SO_4$)	3·05
,, manganese	($MnSO_4$)	0·14
,, calcium	($CaSO_4$)	0·68
,, magnesium	($MgSO_4$)	0·96
Sulphuric acid	(H_2SO_4)	0·22
Chloride of sodium	(NaCl)	7·55
Ferric oxide	Fe_2O_3)	15·43
Silicate of alumina		0·78
Water	(H_2O)	5·40
		99·73

Of the above substances, manganous sulphate was the only one which he had not seen noticed by other writers as occurring in similar situations. The substance was not perfectly stable, the small quantity of free sulphuric acid present producing a slight chemical change. The sublimate gathered from the exterior of the cone by Mr. Anderson differed from the other somewhat in external aspect; one portion was of a yellow colour, the other blue, both having an irregular crystalline structure. As the quantity was small, the yellow and the blue deposit were mixed together. The following shews the composition of the mixture:—

Oxide of copper	(CuO)	7·75
Alumina	(Al_2O_3)	17·34
Oxide of iron	(Fe_2O_3)	1·43
Lime	(CuO)	2·58
Magnesia	(MgO)	1·66
Potash	(K_2O)	4·02
Soda	(Na_2O)	3·94
Sulphuric anhydride	(SO_3)	25·80
Chlorine	(Cl)	1·90
Fluorine	(F)	Small quantity.
Silica	(SiO_2)	16·90
Water	(H_2O)	17·10
		100·42
Less oxygen equivalent to 1·9 Cl		0·42
		100·00

The substances present were thus much the same as in the previous case, but in different proportions. The chlorine present was exactly in the proportion required to combine with the iron to form ferric chloride, thus accounting for the yellow colour of one specimen; the blue colour, on the other hand, was due to sulphate of copper. After pointing out the difficulty of supposing that the presence of the above-named substances in the volcano could be explained by the introduction of sea-water, he went on to show how their presence might be accounted for. The alkalies would be derived from the lava itself, particularly from the more trachytic or felspathic sort, which is richer in alkalies and contains less of the ferruginous minerals. The alkaline silicates would be partly taken into solution by the interstitial water, which exists in all lavas while in the molten condition. Solution would be assisted by carbonic acid derived from the strata through which it ascended, and possibly also by a sulphur acid. The subsequent reaction of sulphuric acid upon the dissolved silicate or carbonate would

account for the fact of its being found in the form of sulphate. The sulphuric acid, he thought, would be derived from sulphides of the metals, iron, &c. For, though iron pyrites was easily decomposed by heat at ordinary pressures, there was reason to believe it could exist at a high temperature if subjected to great pressure, it being a common constituent of the crystalline as well as of the sedimentary rocks. When the metallic sulphides arrived at a part of the volcanic vent suitable for oxidation, they would lose their sulphur, which would be burned to sulphurous anhydride, and in the hot moist atmosphere of the cone would be further oxidized to sulphuric acid.

Mr. Anderson's paper was illustrated by numerous specimens of volcanic products, a number of which were collected in the year 1866 by Mr. Wünsch, a member of the Society, and by whom they were lent for the occasion.

An interesting discussion ensued, in the course of which Mr. Wünsch and Mr. Ogilvie gave some of their own observations made on Vesuvius, Etna, the Lipari Islands, &c. Mr. Tatlock, the President, and other gentlemen, also took part in the discussion. Some degree of surprise was expressed at the non-appearance of ammonium chloride in the results of Mr. Cochran's analysis; but that gentleman said he had purposely sought for it, and had failed to find any. In one of the deposits Mr. Anderson stated that Dr. Thorpe had found, by spectrum analysis, lithium in sensible quantity.

XIII.—*Livingstone Relief Expedition*, via *the Congo.*

Read before the Society, November 20, 1872.

Mr. James R. Napier gave an account of the Livingstone Relief Expedition, *via* the Congo, from documents with which Mr. Young of Kelly had supplied him. The statements which have been made so public in this country regarding Dr. Livingstone's opinions of the slave-dealers on the East Coast of Africa, by Mr. Stanley and others, convinced Mr. Young that his friend, Dr. Livingstone, would not now be allowed to leave Africa alive if he ventured to return by the Ujiji and Zanzibar route, and that when the news of all

that had been said here since Mr. Stanley's arrival reached Zanzibar, the slave-dealer's would be so embittered against Dr. Livingstone that they would be sure to find ways of starving or murdering him. Mr. Young had therefore generously offered to supply funds to the Royal Geographical Society for an expedition to go out and bring Livingstone home by the West Coast. The belief, strongly impressed on the minds of those who have most thoroughly studied the subject, that the great river Lualaba, which Livingstone has discovered, was not the Nile, and could be no other than the head waters of the Congo, was shared in by Mr. Young. The knowledge also that Dr. Livingstone is going to Lake Lincoln, and the great probability of his being found there, or of the expedition being there before him, and that Lake Lincoln is about 300 miles in a direct line nearer the West than the East Coast, so confirmed Mr. Young's belief that he despatched the requisite funds at once, and receipt of the money has been acknowledged by Sir Henry Rawlinson, who states that the expedition will start before the end of this month. Lieutenant Granby, who has been selected to lead it, appears to be a second edition of Stanley in regard to persevering courage and endurance. The expedition will start from Loanda, on the West Coast, proceeding through Portuguese territory to a point on the Congo, about 300 miles from its mouth, so as to avoid the opposition of the slave-dealers of the coast. From that point it will follow the Congo, according to the judgment of the leader, direct to Lake Lincoln, through an unexplored country. It is remarkable that the Congo, although known to be so very large a river—perhaps the second largest in the world, and second only to the Amazon—has not been explored for more than 250 or 300 miles; and nothing more is now known of its wandering in the interior than was known to Captain Tuckey when he ascended it so far in 1816.

To shew the grounds on which geographers have relied for their conviction that the Lualaba was *not* the Nile, and was most probably the Congo, the translation of a paper by Dr. Behm, published in the November number of *Ocean Highways*, was read to the society. The chief reason assigned was that the White Nile could not contain more than one-third of the water which flows in the great Lualaba. Comparisons with all the other rivers shew that only the Congo and another—the Benue, a branch of the Niger—contain more water; but the time at which the floods happen in the Benue shews that it cannot be a continuation of the Lualaba. The Uelle, a river discovered by Dr. Schweinfurth,

rising apparently in the Malegga or Blue Mountains of Baker, on the west side of Albert Nyanza, flowing to the west, and proved not to be connected with the Nile, precludes the possibility of the Lualaba entering the Nile by the Gazal, even if the levels of the two extremities permitted, which, as at present known, they do not. The question is asked and answered in favour of the identity of the Lualaba and Congo,—If the Lualaba does not discharge by the Congo, where does so immense a river get its supply, for there is not now left so much unexplored country to fill it? A map exhibited by Mr. Napier was constructed from Keith Johnston's *Lake Region*, from a tracing by Lieutenant Granby of his proposed route, and also from a map of Dr. Schweinfurth's, in *Ocean Highways*.

XIV.—*On Flour Mill Fire-Explosions.* By Stevenson Macadam, Ph.D., F.R.S.E., *Lecturer on Chemistry, Edinburgh.*

Read before the Society, and Illustrated by Experiment, December 18, 1872.

Five months ago we were startled by the newspaper account of a violent, disastrous, and lamentable fire-explosion, which occurred in a flour mill in Glasgow. A large building of five floors, besides a sunk flat and attics, had its gable-walls blown out, and was fired from top to bottom. The operations which were being carried on were of the most peaceful description, and were simply the cleansing and grinding of grain and the dressing of the flour. All the processes were proceeding in the most regular and harmonious manner, and yet, in a space of time not greater than what is required to describe it—in a few seconds—two explosions took place; the mill was in ruins, the debris was simultaneously fired, the site of the mill was a roaring furnace, and, lamentable to relate, more than a dozen human beings were killed — in fact, out of twenty-seven work-people engaged in the mill, fourteen were instantly killed, one was mortally wounded, eleven were more or less severely burned, and only one, who was on an outer gangway, escaped unhurt.

The occurrence was so unusual, and at first sight apparently unexplainable, that it called for a searching inquiry, in the pro-

secution of which Professor Macquorn Rankine and I were requested to take an active part. The results of the investigation proved two points,—

First, that there was nothing unusual being carried on in the special mill referred to, which would render the operations more likely to lead to a fire-explosion than in other mills elsewhere; and second, that fire-explosions of an exactly similar nature, though differing in degree, had taken place in other mills in this country and on the continent, though the public and even scientific men were unaware of such. These facts enable me on the present occasion to take the particular case into the general question, and to discuss the subject simply in the light of an important public matter, affecting one of the most necessary branches of technical science. The discussion of the subject will appear all the more necessary when I mention that, under the present arrangements of flour mills, there is nothing to hinder another disastrous catastrophe happening any day, and I really feel it is a duty incumbent upon me to afford every possible explanation of the manner in which such fire-explosions have occurred and may still occur.

The nature of the operations carried on in flour mills may be condensed under three divisions:

1*st.* The cleansing of the grain.

2*nd.* The grinding of the grain, and

3*rd.* The sifting or dressing of the grain flour.

In small mills these operations go on day by day with a minimum amount of risk, so far as fire and explosion are concerned. One or more pairs of stones, enclosed in wooden framework, reduce the grain to powder, and the mixed flour and bran are conveyed by a spout to a bin, from which it is afterwards taken to the dressing machines. Some of the finer particles escape into the atmosphere of the mill during the grinding and dressing processes, but the proportion is comparatively small, and as a rule is not sufficient in quantity to lead to a fire-explosion. At the worst, the fire-explosion will be limited to the pair of stones which may strike fire, and possibly to the meal-spout attached thereto; or it may be localized in a dressing machine when a light has been incautiously brought near the apparatus when in full work.

In a large mill, on the other hand, the danger of fire and fire-explosion is much increased—for ten, twenty, or thirty pairs of stones may be at work at one time, and each of these pairs of stones may have a box pipe, roan, or conduit attached, which leads to an exhaust-

fan, and the latter, when in operation, sucks or draws all the finer and lighter flour-dust up into a receptacle called an exhaust-box. The practical results of this exhaust arrangement are, first, that the mill itself is kept more free from flour-dust floating in the air, and thus the men can work in a purer atmosphere, and second, that it accumulates all the fine and almost impalpable flour-dust in the exhaust-box. Generally, the exhaust-box is connected with a second dust receptacle, called a stive-room, where the fine flour-dust which has not been deposited in the exhaust-box is received. Occasionally, the exhaust is assisted in carrying away the fine dust by a blast or system of air blowers.

Now the tendency of the exhaust system is to centralize and accumulate in the exhaust-box and its connecting stive-room all the fine impalpable flour-dust which does not readily fall by its own gravity through the meal spouts leading from the grinding stones, and consequently to store up for the time being those portions of the flour which can be most readily suspended through the air and form a dusty atmosphere.

Simultaneously with the introduction of the exhaust, there have been introduced improved cleansing machines, including decorticators and smutters, for the prior treatment of the grain, and improved sifting or dressing machines for the more thorough separation of the different sorts of flour. All these improvements in milling have been carried out for the very praiseworthy object of perfecting the process, and of obtaining purer and probably better flour, but I am sorry to say they have, at the same time, brought about a condition of matters in many of our large flour mills which may at any time lead to fire-explosion, as in fact has already taken place in several mills.

The chemistry of the grain and flour may assist us in arriving at an understanding regarding these fire-explosions. The chemical components are principally starch and gluten, with small proportions of gum, sugar, oil, woody fibre, and ash. The starch and woody fibre are composed of carbon, hydrogen and oxygen ($C_6H_{10}O_5$); so are also the gum, sugar, and oil; and the gluten contains these elements accompanied by nitrogen, sulphur and phosphorus. All these proximate constituents—viz., starch, gluten, woody fibre, sugar and oil—are combustible when burned in the ordinary way, and are consumed with greater rapidity when diffused as a cloud through the air.

When the flour is showered from a sieve placed some distance above a gas flame, rapid combustion takes place, and indeed the

flour burns with explosive rapidity, and the flame licks up the flour-shower somewhat in the same way that it flashes through a mixture of gas and air, or that it travels along a train of gunpowder. Similarly, the flour blown into a cloud by a pair of bellows at once takes fire and burns with a sudden and vivid flash. The smut taken from the grain during cleansing, and the sharps separated from the flour, burn with equal readiness. Probably the best way of shewing the explosive combustibility of the flour particles, is to place some flour in a box lying on its lid, introduce a light, and blow the flour into a cloud by bellows, when instantly the box is lifted from its lid, and much flame rushes out. The fine division of the flour has necessarily much to do with the rapid combustion; and, indeed, coarse gunpowder can be passed through a flame without burning, while fine iron filings cannot fall through the same flame without being set fire to.

When bruised, the flour resolves itself into gases. The carbon, by uniting with the oxygen in the air, becomes carbonic oxide (CO) or carbonic acid (CO_2), and the hydrogen and oxygen become water vapour, or steam (H_2O). The volume of these gases is much increased by their high temperature at the moment of combustion.

The conditions required to bring about a flour-explosion are somewhat similar to those which cause a gas-explosion. Ordinary gas is combustible and not explosive, when unmixed with air, and it only becomes explosive when it is mixed with sufficient air to burn it. This proportion is obtained in the most powerful degree when one volume of gas is mingled with ten parts of air. A lesser or a larger proportion of air lessens the power of the explosion by causing the gas to burn less quickly, and consequently with less explosive force, and thus tending more to ordinary combustion.

The flour agrees with coal gas in being simply combustible when unmixed with air, and equally agrees with coal gas in being explosive when mingled with air in the proper quantities. The fine and impalpable flour-dust must be diffused through the air in definite proportions in order to constitute an explosion, when a white heat, such as a flame or spark, is brought near, and too little or too much of either of the components (flour and air) of the explosive mixture must lead to the lessening of the explosive force, and to the more gradual combustion of the flour-dust.

In order to bring about the explosion, it is necessary also that the flour-air mixture be more or less confined within a given space,

and hence, if the flour be encased in a box with the lid fastened down, and the bellows be brought into play to produce a cloud whilst a light is introduced, the box will be forced open with a loud explosive noise, and the side be split up, whilst weights placed on the box are thrown off. In fact, an explosion is brought about which resembles the effects of gunpowder, gun cotton and coal gas. All of these will burn comparatively in a quiet manner, when fired in open vessels, and it is only when enclosed and resistance is offered that the noise and effects of a true explosion are observed. No doubt gunpowder and gun cotton differ materially from the other explosive agents, as both the gunpowder and gun cotton contain within themselves all the oxygen or air required to burn them, and thus do not need the admixture of air so essential to the explosion of coal gas and flour.

The order of explosions in flour-mills may now be referred to. The more common way of the production of the spark or flame which fires the flour-air explosive mixture is the feed going off the stones during work, when the stones set down on each other, and as they are of a flinty or hard siliceous rock, and are revolving at from 100 to 160 revolutions in the minute, they quickly strike fire and become very hot.

The feed may go off from want of grain in the hopper, or from any obstruction in the feed-pipe. A spider's web actually stopped the feed in one case, and led to a violent explosion in an English flour mill, where a detached stive-room luckily suffered most. Even when the feed is kept on, a nail finding its way into the hopper and down between the stones instantly strikes fire, and the minute particles of molten combustible iron inflame the flour-air mixture, and cause an explosion, as occurred in one of our Scotch flour mills. Probably the greatest danger is where the feed is suspended for a short time. The stones settle down and strike fire, and then the feed comes on again, in which case much inflammable flour-dust will be instantly produced and fired, and the exhaust will suck in the flame through the roans or conduits leading on to the exhaust-fan and exhaust-box.

In several flour mill fire-explosions, the crackling noise of a running fire has been distinctly heard traversing the roans or conduits leading from the stones to the exhaust-fan. In some cases the explosion goes no further, and may simply blow open the fan box cover, as in one case in Scotland. If the flame passes the fan, however, it gets into the exhaust-box, and every thing depends upon the momentary condition of the exhaust-box at the time, whether

the explosion ends there or the mill itself is blown up and fired. If the dusty atmosphere be at a minimum, the explosion will be comparatively feeble, and will probably end in blowing out the side of the box, destroying some of the windows of the mill, slightly scorching a man or two, and loosening some timbers and slates, besides charring all wood-work within reach of the flame. But if the mill is in full working order, and much dust is being carried into the exhaust-box leading to the flour-air mixture being abundant, then the explosion will be violent, the sides of the box will go, and the fine impalpable flour-dust lying on the floor and shelves will be blown into the atmosphere of the mill, when a second explosion will occur, and the mill itself must go—not only from one floor, but the explosion of the exhaust-box will cause a disarrangement of all the air courses, and the mill being in communication on all floors by means of elevators, hatchways, and other passages, and the cleansing and sifting processes proceeding at other parts, there must be a simultaneous production and firing of more or less flour-air mixture in all the floors, and consequent explosion. A period of darkness, caused by the flour-dust being diffused through the atmosphere of the mill, has been distinctly observed before the main firing of the mill.

The connection between the exhaust-box and the stive-room has much to do with the direction which the explosion takes. Thus, if the mode of communication be by a wooden roan of small size—say a foot in diameter, as in many mills—the force of the explosion in the exhaust-box cannot spread itself in the direction of the stive-room, and must burst the sides of the exhaust-box and force the explosive flour-dust mixture into the mill itself. But if the communication is capacious enough—say 3 or 4 feet square—then the explosion in the exhaust-box will find vent into the stive-room, and the latter, instead of the mill, will suffer. In an English case, where the latter conditions were carried out, the walls of the stive-room were blown out and the building wrecked, whilst the mill itself escaped with some charring of the wood. The stive-room in this instance was outside the mill, but the safety derived from the comparative free communication between the exhaust-box and the stive-room would have been entirely negatived if the stive-room had been simply on another floor of the mill—as I have seen in one of the larger flour mills in Scotland.

The chemical or physical results of the fire-explosion may now be considered. An ordinary size of exhaust-box is 18 feet long, 9 feet high, and 7 feet wide. The capacity of this box will be

1,134 cubic feet, of which fully a fifth, or 230 cubic feet, will consist of oxygen. The quantity of coal gas required to mix with this air, so as to form the most powerful explosive compound, is about 113 cubic feet, or, taking the density of the gas at 500 (air = 1,000), less than 4½ lbs. of coal gas. We all know how disastrous such an explosion would be, and yet only 4½ lbs. of coal gas would take a part in it. With flour dust, however, diffused through the air instead of the coal gas, the proportion of oxygen contained in the box—which would be about 17½ lbs. weight—would instantly burn 28 lbs. of the flour dust into carbonic oxide and water vapour or steam, being fully six times the proportion of the coal gas which could be consumed with the same amount of oxygen.

The sudden combustion of the flour-air mixture at a white heat necessarily produces great increase in the bulk or volume of the gas, and when the whole conditions required theoretically for the production of the most disastrous explosion are practically realized in a mill, the increase of pressure is equal to eight atmospheres, which must necessarily bring about a violent explosion, and cause the destruction of any ordinary building.

The precautions to be taken in the future for the avoidance of flour mill fire-explosions must necessarily lead to the removal of exhaust-boxes, stive-rooms, smut-rooms and other receptacles of flour dust to the outside of the mill, and it will be better if they can be placed at a side of the mill where there are no windows. There should be no direct communication between the mill and these places, except by the comparatively small spout or roan conveying the dust from the cleansing, grinding and sifting processes; and it will be better if the roans be provided with sliding or hinged valves, which would close at once if an explosion occurred in any of the dust receptacles leading to a back draught.

The great sources of danger are undoubtedly the exhaust-boxes and stive-rooms, and these should be constructed either entirely of boarding, so that the explosion might readily find vent by bursting open the planks, or if built of brick or stone, the framework of the windows or other compartments should be loosely set in the building, so that they would readily give way when an explosion occurred. There are mills in Scotland more dangerous in their construction than those in which accidents have occurred, and I have one mill in particular in my mind at the moment, where the exhaust-box is on one floor and the stive-room is on the floor above, and both form part of the respective floors of the mill, and are only

separated from the other parts of the mill by a wooden partition. If the necessary train of combustible and explosive dust leading from the stones through the exhaust roans and fan, exhaust-box and stive-room be complete through the full working of the mill, and a pair of stones strike fire from any cause, the mill I refer to can hardly be expected to escape a more or less disastrous explosion.

Not only should the exhaust-box and stive-room be outside the mill, but the danger will be lessened if the exhaust-fan be also placed outside. It can readily be put in the entrance of the exhaust-box, or in a corner thereof. Should an explosion then occur through the stones striking fire, it will be limited to the exhaust-roans, so far as the mill itself is concerned, and the fan will tend to suck the flame out of the mill. No one should enter the exhaust-box or stive-room during the working of the mill, and the cleansing out of these places should be carried on when the stones are not grinding.

No naked flame or light should be taken into any of the dust places, and all gas jets and lamps should be protected with glass and wire-gauze coverings. Naked flames often lead to minor explosions in flour mills, such as the firing of the dusty atmosphere in the dressing machines, and in some cases in the mixing rooms, where the different sorts of flour are ultimately incorporated together.

I trust that this paper will awaken the proprietors of all flour mills to a sense of the necessity of adopting all precautions and measures to arrest flour mill fire-explosions for the future. The plea of ignorance of the explosive nature of the flour-air mixture cannot now be pleaded. I do not blame millowners, however, for their want of knowledge in the matter of the powerful destructive force capable of being excited by a flour mill fire-explosion, for even science had not uttered a warning cry; and I know that the ablest scientific mill engineer in the country, when asked for his experience and opinion in regard to such explosions, at once replied, that he never knew of a mill being blown up by a mixture of flour and air, and that he did not consider it likely or probable that such could be the cause. Even the Chemist of the War Department was impressed with the novelty of a mill being blown up and fired by flour-dust diffused through the atmosphere of the different floors, and wrote me for the fullest particulars. And to be candid, when I entered upon the enquiry, I scarcely entertained the idea that a flour mill could contain such elements of destruction within

its ordinary operations, and it was only after organizing and carrying out many experiments, and fully considering the whole subject, that I was forced to the conclusion, which theory as well as experiment can now substantiate, that the ordinary arrangement of a modern flour mill render it liable any day to explosions of a more or less disastrous nature.

XV.—*On British Weights and Measures. The Pound or Libric System.* *By* JAMES R. NAPIER, F.R.S.

Read before the Philosophical Society of Glasgow, January 8, 1873.

THE frequent use of the Metric System of Weights and Measures by members of scientific societies, and especially by one member of our own, led me to inquire if nothing British could be found to answer his purpose and theirs. A remark, also, that all nations were adopting it, and that it would be worse for the British the longer its introduction was delayed, made me more determined to know why we should follow France, seeing that I disbelieved that she alone possessed the only thing in nature suitable for measures, either national or international.

A select Committee of the House of Commons was appointed, in 1862, to consider "the practicability of adopting a simple and uniform system of Weights and Measures, with a view not only to our own internal trade, but to facilitate our trade with foreign countries." This Committee reported unanimously in favour of the Metric System; other Committees of the House may have made similar reports. But I am not aware that any commission of the most talented and scientific men of the country has yet been appointed to investigate the whole matter, and lay before us a complete and comprehensive scheme which we might boldly face, and defy the whole world to produce a better.

The House of Commons may be both talented and scientific, but the questions which the Committee of 1862 asked the Astronomer-Royal and the late Mr. De Morgan, shew that they had prejudged the case. They did not ask either of those eminently scientific men, nor, as far as I know, did they ask any other witness, if they

could produce a British System having Metric qualities, or if they could produce anything better, but concluded, unscientifically, that we must follow the incomplete French system; and as they did not look as they were directed to do for a simple and uniform system for our own internal trade, and limited their idea of "foreign countries" to those only which used the Metric, when they might with more propriety have assumed Burns's aphorism, that "Freedom and whiskey gang thegither," we have yet to learn what is best.

As far as I know, or have been able to read on the subject, I am not aware that any one except the Astronomer-Royal for Scotland has seriously tried to find a really international system, one which all the nations of the earth would sooner or later adopt, on account of its superiority to all others. And his Pyramid System has not yet been looked at; for those who ought to have discussed it in the society where it was proposed, left the main point unanswered.

A society at one time styling itself the Decimal Association, with other designations since, one of them being international, set itself to put our Weights and Measures in order, but proposed nothing original. They could see no good in anything but the Metre and Metric System, and went wholly in for it. They got Committees of the British Association appointed, made reports and mural standards for the advancement of science, did a deal of talk, published lots of decimal and metrical literature, and at last persuaded a British parliament to impose "The Permissive use of the Metric System of Weights and Measures Act," and would, if they could, make the use of that system compulsory. Mr. John Joseph Griffin of London, in 1851, came very near to a system as good as the metric, by his decimal divisions of the pound and gallon; but for reasons best known to himself did not complete it, although he states that he was aware how it was to be done—as no doubt every one must soon be, who understands what is wanted and sets himself to find it.

That element of the Metric System, however, on which the proposal I have to make is founded, and which makes that system so attractive to those who understand it, is, I believe, not generally understood in this country. The Decimal Association loved the metre, and advocated it chiefly for its so-called natural base, the ten millionth part of the distance from Pole to Equator; most of the witnesses in 1862 for its decimal divisions, and very few for the only valuable quality which our foot, pound, and gallon system did not possess—viz., its one base quality. The so-called

Decimal System is not by any means the same thing. We may have decimal yards, feet, or inches, decimal weights, and decimal measures, without having the simplicity of the French system; but it has not yet been shewn by any one that we can have that simplicity without these decimal divisions, or divisions having the same base as the integers. That the base ought to be the same as our numerical language is evident. We might, however, with new words have a different base from the common denary one; but it must be continuous, not mixed up with other bases, as our feet and inches, pounds and ounces, gallons and ¼ gills. The metre, the length of a rod in Paris intended to be of the length above named, is then the French standard of length divided into integer powers of 10 parts. The volume of a cubic tenth part of this metre is their litre, and the weight of this litre of water is their kilogrammic weight.

When the volume or capacity of a body was known in decimetres, the same figures express also the weight in kilogrammes, of the same volume of water, or the capacity in litres. When they had found, for example, the displacement of any of their ships in cubic metres, the same number expresses also its weight in their tons; there is no division of that number by 35·94 (cubic feet of water in a ton), or approximately by 36. Or when they knew the weight of their ship, they have no need to divide by 62·355 (weight of cubic foot of water) before they can determine the draught of water. French plumbers can know the number of litres a tank will hold by the simple product of the length, breadth, and depth; whereas a British plumber, before he can know the number of gallons his will hold, has to multiply the product of the dimensions by 6·23, or approximately by 6¼.

When the French had determined to abandon all their old measures, and make an entirely new system, they could begin that anywhere; they had only to make the elements of the new system harmonize with whatever base they adopted. They commenced by fixing the length of their metre, and from that got their litre and kilogramme, as already stated. We, however, having already a pound and gallon, neither of which we wish to alter if we can avoid it have to find a length which shall have metric qualities: the gallon having been by our lawmakers, in 1824 or 1825, made the volume of 10 lbs. of water, the tenth of this, or the volume of one pound of water, is the British litre. It is left for us, then, only to determine its volume in terms of our present standard of length, when ten times

the cube root of this volume will give a measure of length, which with the pound and tenth of a gallon as elements, and each divided into integral powers of 10 parts, will give a British system as complete as the French. This length is 10 $\sqrt[3]{27{\cdot}7274}$, or 30·267 inches. The legal gallon was 277·274 cubic inches; and as the Astronomer-Royal says that it is so still, I am for this purpose content; but as that does not appear to me to be self-evident from the Act, I have given the weight of a cubic foot of water above, from authorities given by Professor Miller, in his paper on the construction of the pound. By taking 10 pounds and 1 gallon as elements, also 100 pounds and 10 gallons, we can find by the same means other two standards of length, either of which would be as simple as the French.

The new measure, 30·267 inches long, may be called a Gradus, Grade, Gressus, or Gress, seeing that it is very nearly the old Roman step, and old English Gressus. The passus being 5 feet, the gradus was $2\frac{1}{2}$ feet, the name suits, although the Roman foot may have been different from 12 inches. The divisions may be called decigrade, centigrade, milligrade, &c. The interval between the centigrade divisions, being about three-tenths of an inch, would, if left undivided, be undoubtedly halved and quartered. To prevent the quartering the interval is divided into five parts, ten being too minute for ordinary use. Each division, the 500th of a grade, is therefore a little less than $\frac{1}{16}$ inch. The rule may therefore be read thus:—16·35$\frac{3}{5}$ grade = sixteen grade thirty-five and three-fifths; or 16·356 grade = sixteen grade three fifty-six. A cubic grade of water weighs 1,000 lbs.; 10 cubic decigrade is the capacity of a gallon. A tenth of the volume of any body in decigrades is its capacity in gallons; and the volume of any substance in decigrades by its specific gravity is its weight in pounds.

The advantage of such a rule to shipbuilders has already been alluded to. The draughts of ships, which are usually made to the scale of $\frac{1}{4}$ inch to the foot, or $\frac{1}{48}$ of full size, would by the new rule be on the scale of 2 centigrade to the grade, or $\frac{1}{50}$. For shipowners, grade callipers would give the weight of any package, as water, and save them some labour in estimating what the freight ought to be. And for whatever purpose the metre is found most useful in France, for the same purpose will the grade be found most useful in Britain, whether that purpose be science or art.

To make this Libric system, as we may call it, in contradistinction to the metric, simpler, the tenth of a gallon ought to be the unit of capacity, when the following might be the nomenclature of some

of the denary divisions, derived partly from parliamentary commissions, old sources, and present use:—

1,000· . .	Milpund . . .	Milner	
100· . .	Cental . . .	Centner	
10· . .	Deck, or decker .	Gallon	
1· . .	Pound . . .	Pinter .	Grade
·1 . .	,, . . .	,, .	Decigrade, or Palm
·01 . .	,, . . .	,, .	Centigrade
·001 .	Millet . . .	,, .	Milligrade

It appears to me that our laws are defective in permitting articles to be sold by any number of a legal standard. It would simplify all business to make it illegal to sell by any other unit than an integral power of 10. Thus:—

$$10^{-3} \cdot 10^{-2} \cdot 10^{-1} \cdot - 10^{0} \cdot 10^{1} \cdot 10^{2} \cdot 10^{3}., \text{ \&c.}$$

Iron, for example, or coal might be lawfully sold by the 1 pound, 10 pounds, 100 pounds, or 1,000 pounds, but not by the cwt. of 112 pounds; the ton of 2,240 pounds, or waggon of 2,688 pounds, &c. I find that this is also the recommendation, in 1841, of the "Commissioners appointed to consider the steps to be taken for the restoration of standards of weights and measures." (See *Abstract of Report, &c.*, 1855.)

All articles at present sold by the ton ought clearly to be sold by the 1,000 lbs., under a new name. The Americans would readily understand this change, for their ton is 2,000 lbs. It is difficult to see that the public would be in any way more inconvenienced by this re-arrangement of weights, than they would by a new postage law, or by any other law that was for the general good. The saving of time and labour in weighing articles, with the saving of time and greater accuracy in accounting, would very soon pay every one for the expense of supplying themselves with the new weights. The Commissioners above referred to estimated, in 1841, the quantity of 14 lb. weights and their multiples then in use as 30,000 tons, and the expense of changing these probably £150,000; but this sum, or the double of it, need not frighten the British nation, if they think it necessary to pay anything; for it is evidently a much better investment than anything they are publicly interested in. Moreover, as there does not appear to be any law against the use of these decimal weights at present, the large manufacturers and consumers might surely meet and agree to accept and act upon so simple a means of saving time, trouble,

and uncertainty. Or if it is illegal to sell by them, the Liverpool grain merchants must have found some means of evading the law when they use the cental.

Perhaps I might agree with the statement made by the Astronomer-Royal in 1862, that, "if he had a new nation to create, with a new system of weights and measures, he would give them the binary scale throughout." It would be easy and slow. I would especially agree with him if he refused to allow his people to invent, manufacture, and use dividing engines. But it appears to me to be a return to barbarism and uncivilization now, with our mechanical skill, to combine this scale with the denary, and continue and extend it as we are doing, for we have recently had added to our measures a $\frac{1}{4}$ gill, being a gallon halved seven times. I submit that it would have made very little difference to the Irishman's drink if he had been obliged, for want of a legal measure, to take the hundredth of a gallon instead of the hundred and twenty-eighth. And he might have been content with a still smaller drink from a legal measure of the two hundredth of a gallon, if it existed. Moreover, when the Commissioners of Standards, in their second report, 1869, state as a reason for recommending this new measure, that it is the natural inclination of the mind to halve and quarter continually, I submit that it is not so very natural to do so where machines have already divided the article into any number whatsoever. It is only the interval which is left undivided which it is so natural to halve or quarter by the mind. Moreover, I venture the assertion that the Irishman could not, by any exertion of his mind, distinguish the difference between $\frac{8}{1000}$ gall. and the $\frac{1}{4}$ gill, or $\frac{1}{128}$ gall., the one being in the denary scale and the other in the binary. If it was for the Irish publican's benefit that this binary $\frac{1}{4}$ gill has been made a legal measure, it appears to me that he would be less likely to err in the cost of his liquor by taking $\frac{8}{1000}$ of the price per gallon, than by halving that price seven times, or by dividing it at once by 128.

Again, when the Commissioners of Standards, and all who have written on the subject, are so unanimous in desiring that all our weights and measures should be in the denary scale, it is remarkable to find that we have recently two new measures in the duodenary scale—the imperial bottle and half bottle, of $\frac{1}{6}$ and $\frac{1}{12}$ gallons respectively. It is to be hoped that some enterprizing bottlemaker, brewer, or wine merchant, will shew their appreciation of the denary system by making and filling denary bottles; and that our lawmakers will for the future refrain from giving

imperial names to any measure or weight which is not an integral power of ten standards, until they have established the principle that some other scale than the denary, whether it be continuous or mixed, is better.

Since I have now shewn the construction and use of the Gradus Rule, I submit that it lies with our lawmakers to get it reported upon with the view of making it a legal standard of length, in order that those trades for which it is applicable may get the full value of its economy—for it is as good as any other stick for measuring a distance, and better than all others for the building of British ships, and for many other purposes besides. Those who desired the metre on account of its decimal divisions, and those who desired it on account of its one base quality, have their wants supplied in this rule without the annoyance of having to change their pound and gallon as well. Land measures alone would be affected by its compulsory adoption, as these would all require to be re-arranged. But at present compulsion is not desired, by me at least, but permission to use *material* standards of that length; for it detracts from its use to be obliged to mention its length in yards, feet, or inches, in every statement, as appears to be required by our present laws. And at the same time it is also necessary, if it is not legal at present, that it be *made* legal to use these weights in powers of 10 by distinctive names, whatever these may be. If a simple and uniform system of weights and measures for our own internal trade is still as much wanted as it was in 1862, it is clear that we can have it with this Libric system whenever we please, and with a great deal less inconvenience than by adopting a foreign system, no better than our own would then be. This would put us equal with the French and all other nations. We could then take a leisurely view of the question of international weights and measures. It is most natural that the French should desire all nations to adopt their metre. It has taken them 80 or 100 years to introduce it; and the probability is that they would positively refuse to change to any other now, however good that might be shewn to be. But that is no reason why we should adopt theirs, when we can have as good of our own. Moreover, their system is incomplete. They have not gone to sea with their metres; their sea-mile is probably the same as our own sea-mile. And since they failed in their attempt to introduce sextants, time-keepers, angle-measuring instruments, &c., divided on the denary base, it will be time enough for us to think of following them when they have made this change, or for us to adopt the

metre, or something like it, when we are prepared to take that step or some other along with it.

To British men of science I submit that this rule has at least as much claim to their attention as the metre, and hope that the President of the Royal Society, with his late colleagues of the Standards Commission, for whom he speaks in his last address, will for the future consider themselves Britons, and wait till the French do what the Romans did of old, ere they again make the statement that "the metrical system is singularly well adapted for an international language." The country which rules the world will doubtless impose many of its own customs and laws; but we have yet to learn that that country is, or will be, France. At present it happens to be the British ton of 2,240 lbs. which rules in most of the Mediterranean ports. And we have an idea that the British engineer and British merchant will compel British measures to be accepted long before this French system. If our ascendancy at sea at present is due to our free institutions and to the immense economy which the promulgation of the true theory of heat, and the best means of applying it, by such illustrious countrymen as Sir William Thomson, the late Dr. Rankine, and Dr. Joule, Sir William Thomson need not tell us now that it will be worse for us if we do not follow France. We accepted his theory of heat, and applied it with immense success, and we mean to apply it still further very shortly; nevertheless, if he and his scientific friends, metrical or otherwise, Sir George Biddel Airy among them, choose to write in a foreign language, and treat us to measures we do not understand, we shall no doubt regret their loss, while remembering their former works. They may come to find, however, that they will not be long missed, and that the change will be worse for them than for us.

Mr. J. C. Stevenson, M.P. for South Shields, in that famous speech of his in 1871 against the compulsory use of the Metric System, states some things in which we differ from him; and as he may use the same or similar arguments against what I have here proposed, I think myself privileged in criticising his speech now. He said it was natural to count by dozens as a reason for having the denary and duo-denary scale in feet and inches. I have no such feeling. It is natural to count in the scale our mothers taught us, and unnatural and difficult to count in any other, as our numerical language is based on the denary scale. It is most natural to half and quarter what is not already divided. It is most natural to count by the divisions already existing on an instrument, and to

half or quarter what is left. It is unnatural to half or quarter a year, for example; we take the nearest day of the $365\frac{1}{4}$ to the half, and we may half or quarter the remainder. The mixture of scales is, to my mind, a very great nuisance. Mr. Edward Sang, in one of his books on arithmetic, has clearly shewn that it is both simpler and more elegant to put a denary and duodenary statement of feet, inches, lines, all into the duodenary scale, in order to multiply or divide, than to proceed as in ordinary cross multiplication and division. Mr. Stevenson is evidently expert at calculations connected with his own business—he can put his figures from one scale into another with ease; but many cannot, and it is for the many that a continuous and denary scale is wanted, in all our accounting and rules. He has shewn us some simple methods of getting the weights of substances; but, first, they are not so simple as by the Metric or Libric system; and second, they are only approximate, for a cubic foot of water is not 1,000 ounces, but nearly 3 ounces less. Again, dimensions are given to tradesmen according to the rule by which they are to work. Nobody would want boards to be sawn half-inch thick if we had not such a division on our rule. The boards would be sawn to the nearest division on his rule, whatever that rule was. The same remark might be made as to the making of boiler plates of certain thickness; but as these are more correctly obtained of the required thickness, and that thickness more easily verified by ordering them by the weight per square foot, the maker could find out the thickness for himself by any rule he pleased. Such, at least, was my practice. As far as workmen are concerned, one measuring rule is as good as another to them; they have to work to what suits their master. Nevertheless, many of the divisions of the foot-rule are easily recognized with sufficient accuracy for many purposes on the Gradus, although not marked on it; for the 5th of the gradus is about 6 inches, the 10th is 3 inches, and the $\frac{2}{1000}$ or smallest divisions are a very little less than $\frac{1}{16}$ inch; so that Mr. Stevenson's $\frac{3}{8}$-inch boiler plate is $\frac{6}{16}$ inch, or 12 milligrade very nearly. I have no doubt that he will find the gradus more useful in his business than the foot-rule. Nevertheless, it does not possess the shadow of a scientific base—nothing which could make it international, although it has exactly the same amount of this quality which the French metre at present possesses. It is British, and any other nation's which has a British pound and British gallon.

I may refer now to Professor Smyth's Pyramid System, which is submitted by its author, not as a British system only, but as one

for all nations. The ten millionth part of the earth's polar axis, or 50·05 inches, is his standard of length. His unit of weight is 5 cubic pyramid inches of matter of the earth's mean density, and his unit of capacity the volume of one pyramid pound weight of water. The base of the system is sufficiently apparent for all nations to appreciate; but the mode in which his weight-measure is derived is not so apparent or unobjectionable. His reasons for taking $\frac{1}{10}$ (metron)^3 filled with matter of the earth's mean density instead of the whole (metron)^3 is unsatisfactory, for we are blind, and indifferent also, as to its connection with an Egyptian Coffer, and we have no other reasons given for this division by 10. Nor have we any satisfactory reason given for dividing the metron into 50 parts or the half-metron, the cubit, into 25 parts. The earth's semi-axis of rotation being an integral power of 10 cubits, no reason is given why the same scale should not be continued in the divisions of the cubit—no reason shewn why they should be quinquinary. We have no objections to the quinquinary, or to any other scale, if it is continuous, and which recommends itself as being so much better than the denary that the individual or nation that adopts it will surely lead. But as Professor Smyth does not propose a continuous scale, but denary integers with quinquinary divisions, I submit that his cubit as a measure is, if at all, only a little less objectionable than our duodenary foot-rule. The objections to this rule not being in the twelve divisions for these, are better than ten; but that, as our language happens to be denary, the integer feet are not in the same scale.

I am willing to admit that this system may have been that employed by the builders of the great Pyramid, and that the inch, five inches, and cubit, as Mr. St. John Vincent Day has informed me, may all have been discovered, but cannot allow that it has any claim to be considered now either as a national or international system, other than its own intrinsic value shews it to be better than other systems—better for the majority of those who would have to use it now.

It is a matter of opinion, not yet tested, as to which of our measures would be most willingly sacrificed or altered for an improved national system, or for an international one; for I deny the statement entirely that we cannot alter a nation's ancient weights and measures. The nation or individual who refuses to adopt any improvement is simply left behind to starve and die out, and the improvement goes on without them or him. Who now spins and weaves by hand for their own domestic uses? Has

machinery not changed every thing and every body? In the Libric system the yard and all its divisions is sacrificed, in the belief that at least two-thirds of the population would prefer it to the Metric system. As a British system, I conceive that the pyramid one is better than our present system—as much better, as the heaviness and volume of substances is more easily ascertained by it, and as divisions by 25 are more easy than by 12. The inch and acre are unaltered, and the other measures but slightly. I have no need to compare the Libric and Pyramid system further for British purposes, as the sole object I had in view was to shew that we could help ourselves to as good and perfect measures as we pleased, either for science or for art, without French aid.

As an international system, it appears to me that the Pyramid one is too manifestly British to be acceptable to any but British, or to such nations as still adhere to a measure equivalent to a British inch. The inch is ingeniously saved, but with the sacrifice of that which most nations, and especially those who have changed their system, consider at least as important—viz., a continuous scale. Moreover, if this continuous scale was no part of the cubit or metron of the pyramid builders, it shews their ignorance of a great improvement in numerical notation.

Although I have been led much farther into this matter of scales and Pyramid measures than I contemplated when I proposed to describe the Gradus, I desire to say a few words in reference to the ancient Babylonian system of numeration. When I was under the impression that Professor Smyth's cubits, with the cubits' divisions, must, to be perfect, be recorded on the quinary base, I applied to Sir Henry Rawlinson to know if there was any appearance of a quinary system in ancient Babylonia; for, if there were, translators might have erred in calling a quinary 10 a denary 10; and so the ark might have had the proportions of a moderate sized hulk, instead of those of a magnificent sailing ship, and patriarchal ages would be greatly reduced, &c. But of this there is no evidence, as Sir Henry's letter shews:—

"21, CHARLES STREET, BERKELEY SQUARE,
"*26th December, 1872.*

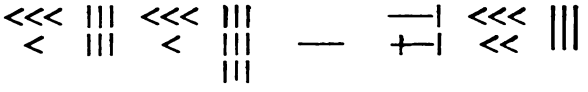
"There is no indication of a quinary system of notation in any variety of cuneiform writing, though the number 5 was much affected, as in the division of the month into 6 weeks of 5 days each, the 5 planets (exclusive of sun and moon), the 5 stars or pleiades, &c. The primitive Babylonian system was sexagesimal—that is, years were reckoned by the Sar, the Ner, and the Soss—the Soss being 60, the Ner 600, and the Sar 3,600; and this alternation of 6 and 10 seems to have pervaded all numerical calculations, whether applied to time

or measures of weight, distance, or capacity. In later times, the ordinary system was entirely decimal, up to 1,000,000 at any rate, as the numbers of fighting men are not unfrequently given at some hundreds of thousands. There is not a shadow of a doubt as to the system being denary, and not quinary, as you may satisfy yourself if you will refer to a table of squares which I published, as an example of the Babylonian notation, nearly twenty years ago. (See *Journal of the Royal Asiatic Society of London* for 1854, p. 4.)* The sexagesimal system was of very great antiquity. The clay tablets in the British Museum, which contain examples of it, are copies of original documents, dating in some cases as high at least as 3,000 or 4,000 B.C. I believe myself the historic period of Babylon to go back as far as 5,000 B.C.; and there was a mythical period before this of at least the same duration.

"As for the Biblical numbers before the time of Abraham, I do not think the least credit is to be given to them. The numbers differ in every copy—Greek, Hebrew, and Samaritan—and are all evidently the work of chronologists! But the *Toldoth Beni Noah* is, at the same time, of extreme value as a genuine work of primitive ethnology.

(Signed) "H. RAWLINSON."

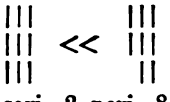
The following may be taken as an example of the table of squares referred to; British characters being used here instead of the cuneiform of the table.



The number represented being—

Soss.	Units.		
4 6	4 9	=	$(5\ 3)^2$

The denary number 34080, which, in the sexagenary scale is 9280, would be represented thus—



which Rawlinson would read as

9 sari, 28 soss;

or, 9 sari, 2 neri, 8 soss. They appear to have had only two characters, viz., a < or 10 and a 1. It is not evident from the table alone whether 1 is a unit, a soss, or a sare; for there appears to be no character for 0, and local value is not apparent. The scale however, is evidently sexagenary.

60 units make a soss.
60 soss ,, sare.
60 sari ,, ?

The arrangement of the scale may be better seen in the abacus form in which the denary number 34080 is represented.

* Vol. xv., 1855, (J. R. N.)

SARI.		SOSSI.		UNITS.	
	3600 to 36000	600 to 3600	60 to 600	10 to 60	1 to 10
	||| ||| |||	<<	||| ||| ||		

We have here precedent at least for a longer scale than 10 or 12 or 25. I have referred more particularly to this scale, from my attention having been directed within the last few days to a discussion on the subject in the *English Mechanic*, of March and April last. Had I seen it earlier, the probability is I would have written less. I was not aware till I saw E. L. G.'s communication, in the number of 1st March, that any one had mathematically considered the question of scales. The best and only ones possessing the peculiar property that each will divide in as many ways as any number below its double will, he states, are 2, 6, 12, 60, 360, and 2,520; and of these he prefers the 60, and proposes to divide it as the Babylonians did, into 6 and 10. 60 has 10 divisors, 25 has 1, 24 has 6, 12 has 4, 10 has 2, &c., besides 1 and itself. If we had a simple method of working this Babylonian Rule, it would no doubt, be very convenient; for even the continuous denary scale is not an unmixed good. Mr. Edward Sang, in his chapter on *Analysis of Arithmetical Systems Generally*, wherein he examines the duodenary and lower scales, concludes:—"If we look only to facility in manipulation, the preference must be given to the senary scale, because the factors of all the numbers below 6, excepting 5, are also factors of 6; so that all the operations of multiplication are easily performed. The progression of the senary scale is slow, and on the whole, we may admit that the choice of the denary scale has been most fortunate, since, without needing the recollection of an unmanageable set of products, and without involving any great intricacy in work, it yet progresses with a rapidity sufficient to satisfy all the requirements of business or of science."

It is not for me to say that machines cannot be made for making calculations, in scales combined in any manner whatsoever, or such that would work any kind of question involving the variety of scales in our moneys, weights, and measures. But the demand for labour-saving machines has already caused a beautiful one

to be invented for making calculations in a continuous denary scale, which doubtless could be easily altered or made for any other continuous scale. But a simple and cheap machine, to work such a variety of scales as we have, has yet to be invented. The French, adopting the denary scale as a *principle*, enabled Mr. Thomas of Colmar, to construct these elegant arithmométres of his.

It is evident that in a complete British system, the sea and land mile ought not to be so distinctly different as is the nautical and statute mile. Being misled by a statement in the late Dr. Rankine's *Rules and Tables*, that a British nautical mile was, by Admiralty Regulations, the length of a minute of longitude at the equator, I conceived that a better mile could be found about the latitude 30°, which might also be the base of an international system of measures and weights. That a mile taken in a particular direction at the parallel of latitude which divides the earth's surface, between the equator and either pole, into two equal portions, might be tolerably constant, would be common to all nations which used the same divisions of their sextants or angle-measuring instruments. While talking over this matter with Dr. Rankine a few days before his death, he immediately suggested the direction, and said it would be very nearly the length of a minute of meridian at lat. 45°; if so, it is something for British or International Commissioners to think of, and something more in favour of the pyramid as a monument of reference, for there, or near it, this mile might be measured of a length = $\sqrt{\text{minute of meridian} \times \text{minute of prime vertical}}$. It would be for these Commissioners to decide what proportion of the quadrant or circle theminute should be. Although it appears to me without knowing much about it, that it would be a very simple alteration, easy to make, and a very great improvement to divide the degree into integral powers of 10, 10^0, 10^1, 10^2, 10^3. The length of the sea and the land mile would then be about 3,646 feet, and a new international standard of length might be 3·646 feet. The British sea mile however, is defined by Admiral Richards, hydrographer to the Admiralty, as a minute of the meridian at the place: it is therefore a variable quantity, and a true mile only in a north and south direction. The mean minute, or sea mile of the mean of the whole earth is about 6,077 feet.

Another matter of importance to many, connected with a complete system of measuring, is the thermometer. We have Fahrenheit, Reaumur, and Centigrade scales, the last being that preferred by the French, and very often used by British men of science. We all feel a use in marking what is commonly called the freezing point,

but which should properly be called the melting point of ice on our thermometers, and that those who call that point zero have an advantage over us who begin our scale at an insignificant point, degrees below it. But we have yet to learn that there is anything in nature which so clearly indicates that the interval between this melting point and the boiling point of water should be divided into a 100° in preference to 180°, 250°, or into any other number of degrees—so clearly indicates it, that British philosophers again follow the French, as if nothing but a French scale would indicate the truth. We happen to know now, and have known for many years, that the actual heat of a body has no relation whatever to this interval, and therefore that the centigrade division of that interval is a matter of no consequence whatsoever. Our own 180 divisions are better, and 250, as proposed by Professor Smyth, would be still better, for the divisions are shorter. Dr. Joule, Sir William Thomson, Dr. Rankine, and Mr. Herapath were among the first to discover the actual heat of bodies, and by their theories determined the absolute zero of temperature.

Therefore if our thermometric scale has to be altered, we have no need to copy French blunders, when we can invent for ourselves and produce thermometers which shall shew actual heat. We can if we choose divide the interval between the melting point of ice and the absolute zero into 1,000 parts, and continue these divisions upward as far as we please. We shall then have a magnificent milligrade thermometer, vastly better than the centigrade. When writing to Dr. Joule, enquiring who it was who first discovered the absolute zero of heat, and explaining what I proposed, he answered, that the scale which I had proposed was described by Mr. Herapath, in his *Mathematical Physics*, in 1847.* I find that it is so—but the absolute zero is wrongly placed. This, then, is Herapath's thermometer, which I had reproduced with the zero where it should be. And one wonders why Sir William Thomson, and all other British philosophers, who are so fond of treating us to centigrade

* Dr. Joule to Jas. R. Napier.

Manchester, *22nd Nov., 1872.*

I find that I wrote, in 1844, that the zero of temperature is, by the theory proposed, 480° below the freezing point. But the same statement is made in Herapath's *Mathematical Physics*, vol. i., page 286, published 1847; but he probably had been working at it and published his results before. After the law of expansion, &c., of gas was proximately discovered by Boyle and Charles (see Maxwell on Heat), it was natural to put the zero at the point where gas is reduced to vol. 0. Herapath has actually in his book, page 280, given a

temperatures, despise so simple and natural a British division, and one so long known. Sir William Thomson, of all men in the world, ought to have been the first to use it, and to use nothing else, for he it was who first discovered that the maximum work which any heat engine can produce, is simply the ratio of the range of temperature to the highest temperature, or $\frac{T_1 - T}{T_1}$, as shewn by this same thermometer. But it requires the divisor

table, in which he places side by side what he calls true temperature and Fahr. scale. Thus:—

True Temperature.	Elasticity, or Vol. of Gas.	Temperature Fahr.
0	0	– 448
100	10	– 443·2
200	40	– 428·8
300	90	– 402·8
400	160	– 371
500	250	– 328
600	360	– 275
700	490	– 212·8
800	640	– 140·8
900	810	– 59·2
1,000	1,000	+ 32 freezing
1,100	1,210	+ 132·8
1,172·6	1,375	+ 212

So you see his suggestion is the same as yours, only he calculates the intervening numbers wrong. You will find in T. and T.'s * paper, in the *Phil. Trans.* for 1854, part ii., page 353, tables by Thomson, with zero of temperature, 273·7.

In the *Phil. Trans.* for the same year, part i., page 149, Rankine places the zero at —272½ Cent., or 458½ Fahr., but remarks that the position of the zero may possibly be too high by 1½° Cent. Maxwell in his book gives the zero at —272·85 Cent., or 459°, 13° Fahr.

What I have done in investigating these points gives me no claim to have my name associated with your new scale, which I think ought to be called by its proposer—viz., yourself. I think you will agree with me that it is undesirable to make a new scale, unless there should prove to be an absolute necessity for it. My idea about the Metric system is that it has always been a mistake, not on account of its defects, but because no power of government will ever force the British workman to give up his 2-foot rule. With many, probably a majority of people, the complication of a system is quite charming. I recollect that it was hoped that the fact that a Manchester firm had adopted the Metric proved the willingness of the trade to use it; but the truth was that that firm used it because it was not general, and when it became so, they would doubtless go back to the foot.

(Signed) JAMES P. JOULE.

* Thomson and Tait.—J. R. N.

to be increased by 274 for every application of it with the Centigrade thermometer, or $U = \frac{T_1 - T}{T_1 + 274}$ Centigrade.

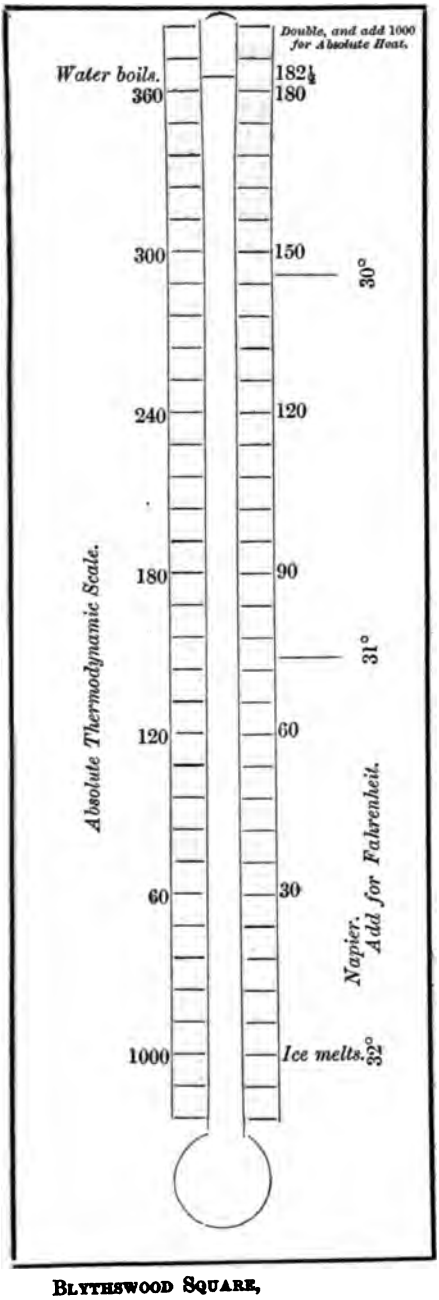
Mr. Clerk Maxwell, in that elegant little book of his on heat, has given us an idea of what this absolute zero of heat means, or may mean in a thermometer so graduated. It is simply the bottom of an air thermometer tube. If the divisions on an air thermometer, between the melting point of ice and the boiling of water, be continued to the bottom of the tube, that point is 274° Centigrade below its zero; is 461° Fahrenheit below its zero; and is 0° or zero Herapath. The simplicity and beauty of the milligrade absolute zero scale of Herapath's is such that all nations may adopt it. It is a rational scale, and we hope that Sir William Thomson at least will abandon those French Centigrade notions of his, and stick fast by a vastly better British one.

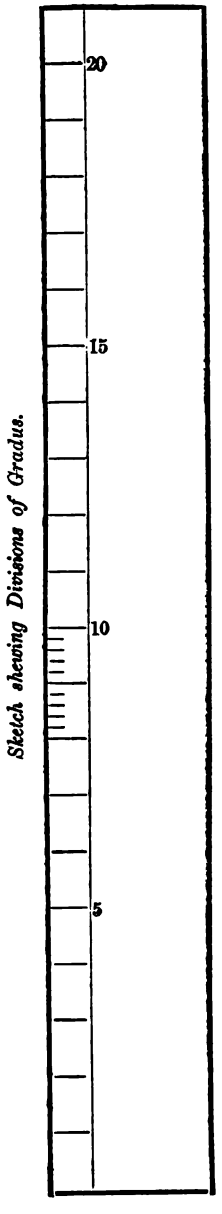
It might at first disturb the feelings of the general public, if told that some one was nearly frozen to death with the thermometer at 1,000°. To prevent this alarm, and to enable the British public to express their feelings of heat in such a scale as to be easily translated into absolute heat, and also enable them to continue the use of that most appropriate expression, degrees of frost, in degrees of of very nearly the same magnitude as those they now use in Fahrenheit's scale, I have constructed another scale which I now submit. When the absolute zero is placed at 1,000° below the melting point of ice, it is found that the boiling point of water is 365° above it; the half of this quantity, 182½°, is so very near the 180° interval in Fahrenheit's scale, that I propose to make that interval 182½°.

No doubt the idea may have occurred to many to raise Fahrenheit zero from 32° below the melting point of ice to that point. But, then, there was no reason why in making that change the 180 divisions to the boiling point might not at the same time be changed to something else, for 180 had no peculiar properties attached to it—none that we were aware of—more than 100 or 250, or any other number. But now we have a reason for desiring that interval to be 182½,—viz., that temperatures by such a scale are converted into absolute temperatures by merely doubling the reading and adding 1,000°, then Fahrenheit degrees are recovered, approximately by adding 32° to T,

and exactly by adding 32 to $\frac{72}{73}$ T,

$$\text{or } T_F = \frac{72}{73} T_N + 32.$$





BLYTHSWOOD SQUARE,
GLASGOW, *January, 1873.*

Note received, January 27th.

I have been reminded that I have not alluded to the late Sir John Herschel's proposals for simplifying our system of weights and measures. I supply the omission now. He would increase all our measures of length about 1,000th part, or such that his new inch, or fundamental unit of length, would be the 500 millionth part of the earth's polar axis; his new cubic foot would then contain 1,000 ounces of water at standard temperature. The half pint, the volume of 10 ounces of water, would be the 100th part part of a cubic foot; the ounce instead of the pound would be his unit of weight; and the volume of this ounce of water, or the cubic tenth of a foot, his unit of capacity. He would make any kind of decimalized denominations which anybody might agree to buy, sell, or contract by, permissive; and he would legalize a measure of 50 geometrical inches, or its half, the geometrical cubit, leaving its use quite voluntary.

This system is evidently as complete and perfect as the metric. Its author, however, had not the courage to go in for such a complete reform as would have prevented the metric system from having been listened to in this country. Perhaps, like E. L. G. of the *English Mechanic*, he had reasons for thinking that the decimal system was not the most perfect form of arithmetic. As I have looked at the subject, I have supposed that it would cause less inconvenience to sacrifice the yard, foot, and inch, than to sacrifice the pound; for in any case a sacrifice of something is needed.

The reference which Dr. Joule's letter gave to Herapath's works has enabled me to discover that he gave a paper to the Royal Society in 1820, on *The Absolute Zero*," placing it at - 448° Fahr.; that his table of true temperatures, referred to by Dr. Joule, was published in *The Annals of Philosophy*, in 1821, with other papers on heat, refused by the Royal Society; that what I have said of Clerk Maxwell's explanation of absolute zero is originally due to Herapath, and about the same time to Laplace, Clement, and Desormes; and that, if I have not admitted Dr. Joule's statement too hastily, what I proposed, in making the interval between the melting of ice and the boiling of water, 182½° instead of 180°, may be also due to Herapath, who made that interval 172$\frac{3}{7}$ of his degrees of true heat.

Discussion on MR. NAPIER'S *Paper.* *8th January,* 1873.

SIR WILLIAM THOMSON said:—The thanks of the Society and of the public are undoubtedly due to Mr. Napier for the great trouble which he has taken in bringing before us the subject of Weights and Measures. I have not the slightest doubt but great benefit will result from Mr. Napier's communication. I am afraid I cannot agree with him in all his conclusions, but to some of them I do most heartily assent. With respect to the milligrade absolute scale of temperature, his observations are exceedingly valuable, and were it not for the difficulty of changing from one or other of the scales to which we are accustomed, I think very great advantage would be derived from adopting the Herapath-Napier thermometer. Mr. Napier's advice to subtract 1,000 from Herapath's numbers is very good. There is, undoubtedly, great advantage in having a round number such as a 1,000 for the addition to be made to the ordinary reckoning to give the divisor that is necessary for calculating the duty of a thermo-dynamic engine. The Centigrade scale, for our preference to which Mr. Napier has been rather severe upon us, is not worthy of any very devoted approbation. It seldom happens that there is any very great advantage in being able to reckon directly in terms of the range from freezing to boiling. The fact that on the scale of 1,000 from absolute zero to freezing, the number is 365 from freezing to boiling, renders Mr. Napier's system exceedingly convenient in its relation to Fahrenheit's scale, because the differences from the exact relation of two degrees to one are so small as not to affect the result in instruments for popular use, and these differences can easily be allowed for in accurate thermometers. I think this suggestion is well worthy of notice, and shall be exceedingly glad to see it adopted. I feel a little compunction about the Centigrade scale, in consequence of its being so different from our familiar Fahrenheit scale, and should like better a scale where we had only to double the familiar differences from 32° to find the temperatures on the new scale.

As to measures of space and mass, Mr. Napier has a great many propositions. I don't know quite which of them to consider serious. One of them I most thoroughly agree with, and that is in preferring the hundredth of a gallon to the hundred and twenty-eighth, if the liquor is good (laughter). I am afraid there are a great many cases in which the hundred and twenty-eighth would be decidedly preferable. Mr. Napier's paper from beginning to

end, and his additional remarks, are all in favour of a decimal system. He has, however, very judiciously given credit to the scale of 60. There can be no doubt of the importance of that scale. Facility for expressing various sub-multiples of a standard is a most important quality of any mode of subdivision to be chosen Hence, subdivision that involves the factors 2, 3, and 4 is preferable to the division which involves only the factors 2 to 5. The duo. decimal system is in itself, for this reason, much to be preferred to the decimal system, were it not the fact that the Arabic reckoning in arithmetic is decimal. Compared with the binary system the decimal system is preferable from having two factors and being shorter, while the binary system has the advantage of simplicity. The decimal system, however, agrees with the fact that we have ten fingers on the hands. As it is impossible to introduce the improvement of twelve fingers, I am afraid we must continue bound to the decimal system. But again, the sexagesimal system has the advantage over all the others in respect of the number of factors; we have 3, 4, and 5,—in fact, we introduce as divisor the next integral number. The inconvenience of the decimal system is, we jump over the 3 to use the 5. The advantage of the sexagesimal system is its use of the 3 and 5. We cannot, however, expect to see the sexagesimal system generally adopted. There can be no doubt, however, that it is from its having these advantages that it has kept its place in our division of the degree of arc. The advantage is so great that I doubt if instrument-makers would like to give up the sexagesimal division of the minute, and adopt the centenary division there. However, Mr. Napier's suggestion to divide the degree of arc into hundredths is undoubtedly well worthy of consideration. I can answer for this, that in many calculations in astronomy—as for instance in the calculation of tides for the British Association—that practice is regularly followed. Mr. Edward Roberts, of the Nautical Almanac Office, regularly does his work on the tides in degrees and hundredths of a degree. The suggestion is well worthy of consideration for trigonometrical tables in general; and I think the sooner we get rid of minutes and seconds the better.

As to measures in general, Mr. Napier said what could be said for the binary system very ably, and for the duodecimal system also; but he falls back on the decimal system. He gives us a decimal system for British measures, which would be beautiful, and all that could be desired, had it two qualities. Were the "grade" a convenient sub-multiple of the earth's quadrant, and were it a

unit already in use by a large part of the civilized world, it would not only have all the advantages Mr. Napier has claimed for it but would have every conceivable advantage. I would very gladly see it compulsorily enforced as a standard for all British measures. The metrical system has, in reality, both these advantages, and it has all the advantages of the decimal subdivision which Mr. Napier has so ably put before us. The metrical system was founded, as Mr. Napier has stated, on the subdivision of the earth's quadrant. I am glad to see that Mr. Napier has not entered upon the futile and ignorant way of arguing that has sometimes been entered upon in discussions regarding the metrical system. Ignorance with the appearance of learning is often put forward, but in a manner to disguise the true state of the case. It has often been argued that the metrical system is not what it professes to be, because we do not know precisely what the ten-millionth part of the quadrant of the earth is. These people forget that there are different quadrants of the earth. The metre was intended to be the ten-millionth of the quadrant through Paris. But what is practically valuable about it is that it is the ten-millionth part of some quadrant, and nearly enough for all practical purposes to the ten-millionth of any quadrant. The late Sir John Herschel put forward as a merit of the inch, that it was the five-hundred-millionth of the earth's diameter. This would be really a merit had we been in the habit of travelling directly through the centre of the earth; but we are much more accustomed to prefer long arcs of the earth's surface than to travel along a diameter.

I think it a misfortune that the founders of the metrical system—of the French system, if Mr. Napier will not be too much offended at me calling it so—it is a misfortune that the founders of the system did not take the nautical mile for their standard; that they did not take the 60th part of the 90th part of the quadrant, that is to say, the 5,400th part of the quadrant instead of the 10,000th part, as their kilometre. They made their metrical system, I believe, with the idea that the quadrant was to be divided centesimally; and if they had carried out that part of the reform, then their system would have been perfect. But that part of the reform was radically a mistake. It would give us a division of the quadrant in which an interminable decimal is necessary to represent the angle whose sine is one-half. Mr. Napier advocates, on the whole, the centesimal division of the degree, but he wisely did not advise the centesimal division of the quadrant. The division of the quadrant must involve the

figure 3, because one of our most important angles is 30° of the nonagesimal reckoning. How exceedingly inconvenient that that angle should be only expressible by an interminable decimal; that the angle whose sine is one-half, or half the angle of an equilateral triangle, should be 33·333. I consider that mathematical truths are too strong for any division of the quadrant which does not involve the number 3. Ninety does include this factor, and has for a long time been used. I see no reason for leaving the nonagesimal division of the quadrant. That was the sole mistake, I think, the French made; in all other respects their system was perfect. The only reason Mr. Napier has given us for the "grade," is that it suits the gallon. The Report of the Parliamentary Committee, which he thinks unwise, many of us are disposed to look upon as an admirable specimen of parliamentary wisdom. I wish all parliamentary reports were as good as that one which Mr. Napier found unwise. I don't say parliament was wrong in fixing the gallon definitely—they were perfectly right. The "grade" measures are not inches. We do not know what they are like, till we look at this (holding up one of Mr. Napier's measuring rods). It is an admirably divided rod. Well, I think it is a more useful one than any I have in my laboratory, because the divisions are more clear. I do think that the change to the British workman, from his two-foot rule to this, would be as hard to introduce as from the two-foot rule to the metre. Mr. Napier has fully explained to you the merits of the system in which the mass of a cubic unit of water would be the unit in weight or mass. He has shewn us how, in shipbuilding, we will need to know the bulk only. In measuring goods we have a corresponding advantage from that condition. On the French system, the ton is simply a cubic metre of water. If shipbuilders want anything more convenient than that, I would like to know what it is. The only objection to the metrical system is patriotism. Well, I do think we need not have any jealousies whatever of a national kind. We have the greatest respect for the long-suffering and admirable spirit of the French. They cannot but call forth sincere admiration. The very Germans, who of all nations of the world might at present feel jealous of allowing ascendency to French genius, have adopted the French metrical scale. Why should we hang back simply because it is French?

Mr. M'Gavin said—I tried practically to introduce the selling of grain by the hundred pounds a few years ago, and a very extraordi-

nary thing happened. Parties who we thought might have preferred it were very much against it. We thought it would simplify business as it was about the same time they introduced the "cental" in Liverpool. Practically it would have been a great advantage to the corn trade. Meantime we are perplexed by so many varied standards of weight and measure that it takes a young man a long time before he can understand where he is. I think if we could have introduced the "cental" at that time it would have been a great advantage. Tens, hundreds, thousands are very easily counted. Unfortunately we have not had it tried for the reasons I have stated.

On the motion of MR. DAY, the further discussion of Mr. Napier's paper was adjourned.

Continuation of Discussion on MR. NAPIER'S *Paper.*
February 5, 1873.

MR. DAY said:—There are several points in Mr. Napier's paper with which I quite concur—there are several also in which I entirely disagree with him—and generally, I think the view he has propounded too narrow to admit of adequate treatment of the questions involved.

I shall endeavour to point this out by explaining what I myself with others consider to be the only true aspect in which to front the proposal of any change in our present metrological system, whether that be for Britons alone or for an International system.

In selecting a radix for a system of standard weights and measures, two modes of procedure present themselves.

First. We may fix upon some arbitrary radix (which may or may not have some special reference to any known thing in nature) as a standard of length; and by adding, dividing, multiplying, squaring, or cubing that radix, erect a whole system of linear, surface, cubic, and weight measure, with which to carry on the ordinary buying, selling, and measuring transactions of daily life. But the moment we pass out of the ordinary or every-day rough and ready uses of weights and measures, and ascend into the region of their more exalted use—that is, for scientific purposes—we are at once hampered by a system founded upon an arbitrary radix, because it will not conveniently admeasure with many of the facts, quantities, or circumstances with which we then have to deal.

Hence, without going into a proof of the truism aforesaid, it may be generally concluded, that it is an improper or inverted mode of procedure, to begin by the selection of an arbitrary radix, and reduce the facts, quantities, and circumstances with which the sciences deal, to its level.

Secondly. As resulting from the foregoing considerations, it follows that the true mode of procedure is to look directly in the face the several purposes for which weight and measure are required, and to carefully weigh the question, Whether the strictly scientific purposes or those which are more or less utilitarian preponderate? Now, a momentary inspection of that question finds an answer overwhelmingly clear, that the scientific purposes demand first attention; and in this conviction some persons have laboured with the view of singling out some specially refined reference-radix.

What, then, is it to which science points as the most fitting foundation for all purposes of a system of weight and measure?—what is best known, and least altering or alterable, either by the hand of man or natural decay—on which to base such a system; which shall provide simplicity in its employment equally to suit the scientific man, the merchant, and, indeed, the least cultured of those who buy and sell; what, in fact, offers the very best foundation to build that system upon, so as to elevate or consolidate the largest as well as the least of business transactions, with strictest simplicity and refined accuracy?

Let us endeavour to realize the position. Stand we at the centre of the "Circle of the Sciences," or as near thereto as we can get, where each science is shedding its own especial Light upon our vision, and do we not perceive instantly, without any doubt whatever, which of the sciences it is that has most to do with weight and measure? Are we not, I say, in a moment convinced that the closest approximation to truth in our astronomical knowledge is absolutely and entirely dependent upon the perfection of vastest weighings and transcendent measurings? that without such weighings and measurings any *real knowledge* respecting the Kosmos and its laws could not exist. To what a sublime zenith, then, do we not perceive that "weight and measure" teach us to soar? and having been carried, thus, so high above the ruts and grooves of daily life, let us review from that height, far stretching though it be, yet, the one only position which limits and includes all things measured and measurable, no less than all men, the features of Kosmos, in order due arranged. Let us mark well from amongst those features, that one which indicates itself to us

as the radix, we, as dwellers on one of the kosmical spheres, are thus directed to choose.

I have just now said that the approach to exactitude of our knowledge of the Kosmos depends upon the perfection of our linear and weight measurings. What, then, is the foundation of all mankind's celestial measurings? Is it not this very Earth-globe we dwell upon? Yet not the Earth itself merely as a spheroidal body, but the angular measures proceeding from extreme points of its radius as a base-line for the obtainment of the angular subtense or parallax of comparatively near bodies—such as the sun, moon, and some of the planets—and the diameter of the great ellipse traced out by that very radius in journeying through its annual orbit, as the base-line for measuring the parallax of more distant bodies within range of our observing instruments, from which again we deduce linear distances.

The French savans, in selecting an Earth-reference standard, adopted a feature which is not directly useful for astronomical measurings, in so far that it was an Earth-surface feature, and one whose value is different at every variation of longitude position. They measured a meridian arc, passing through France from Dunkirk to Formentera, of 12° 22′ in extent: from that calculated the quadrant, from the equator to the pole, in the meridian of Paris, taking the 10,000,000th of that quadrant-length for their linear standard, on which it is well known the Metric system is based.

If it is allowed that the Earth's surface has been sufficiently well measured to give a surface-Earth-reference standard, we may see that an Earth's diameter reference standard is just as attainable, and with no greater error, from observation; because the deduction of diameter from circumference is not a matter of mere observation, but of the mathematics usually called pure and exact.

Some years since, John Taylor pointed out, and Sir John Herschel seconded his arguments, shewing the fitness of the Earth's polar axis as the radix for a standard of measure, on account of its dimensions being better known than those of any other feature which the Earth presented; and although they were at the time sharply criticised for their proposal, yet it has always seemed to me that Taylor and Herschel were the first to recognize the only true foundation of an universal metrical system. Yet, rather because that line is practically a round number of units, with which the present British inch corresponds within $\frac{1}{1000}$th of its own length, was it that Sir John Herschel argued its importance, than because it constituted

the fittest foundation for a standard wherewith to measure all things scientific or common-place, and that on that account it enabled us to maintain our hereditary and precious British inch, and would not involve us in the difficulty of having either a new metrical language, besides giving us a large linear unit, though less in length than the yard, yet possessing qualities of rare and highest convenience for use.

When in correspondence with the late Sir John Herschel, in 1868, on this and a kindred subject, he sent me a copy of a letter of his, published in the *Daily News* of 27th May of that year, wherein he states,—"I consider both the one and the other (meaning by these the meridian quadrant through Paris and the Earth's polar axis) to be actually known in terms of our exisiting material standards, to a degree of precision that no other subjects of human measurement can pretend to; and certainly to a degree superior to that of the determination of the length of the French meridian quadrant, which was adopted by the French savans, and which actually forms the basis of their metric system; though most assuredly I will not venture to assert that the length of the polar axis of the Earth is known to within a single foot out of the forty and odd millions of which it consists—which is more than I think any "Cambridge Mathematician" would like to allow for the periodical variation (if any, *which I doubt*) of that length, due to the annual accumulation and liquefaction of polar ice. Suffice it for the present purpose, that if such, or anything like it, be the degree of precision required in the determination of the length of a scientific unit, it is rather to be desired than to be hoped that he may see it attained."

Proceeding further, Sir John Herschel at the same date says,—"I have nowhere denied that the metric system of the French is 'sufficient in point of accuracy for all common purposes.' Of course, it is so; and equally, of course, so is the British Imperial system, and so is any and every other system which refers all practical measurements to an actually existing and accessible material standard, duly sanctioned by law, and carefully preserved from injury."

Again, at the same date, Sir John Herschel says,—"I *have* asserted, and I repeat the assertion, that a British Imperial inch does differ in defect from a geometrical inch (regarded as one five hundred millionth part of the polar axis), by an exact thousandth part—exact, that is to say, so far as any, the very nicest, scientific (*not* merely practical) requirements, in the present state of science,

can insist upon. And I have insisted, and continue to insist, on the extreme facility which this numerical relation offers to the memory, and to the arithmetical process of converting British Imperial measures into aliquots of the polar axis, regarded as a fundamental unit of linear measure, in the few and rare cases in which such conversion may be desirable, affording in this respect a manifest advantage over the actual practical French metre, as related to its theoretical prototype."

But Sir John Herschel's convictions did not end with the foregoing remarkable comments, for he further on says,—"It is not for 'common practical purposes' that scientific men desire to refer their measures to one or the other of these great natural units or bases. *It is for the purpose of connecting, by an unbroken numerical chain, the small units with which mortals are conversant in their constructions and operations, with the great features of nature, and more especially with those greater units with which astronomy brings us in relation.*"

Now the Earth's polar axis has been well styled as the best known, most accurately placed, and least variable line in nature, and Captain Clarke has shewn it to be 500,000,000 inches, of which one is equal to 1·001 present British inch; and directly it is heard by many persons that the standard so proposed is on a quinary base instead of a decimal one, those who would decimalize everything fall in with objections, and chiefly that a quinary scale is inconvenient for practical use, whereas a denary one is in every way convenient. But I seriously ask, Is it the fact that the denary system is most convenient? or rather, is it not the fact that it has been tried and found wanting? When the French savans devised their metrical system, it was their intention to decimalize everything measurable—yea, even time itself—and so they devised decimal weeks and decimal divisions of the circle; but, whereas it was found in spite of that love of theirs for marking off everything in tens and powers of 10, that nature herself still stuck to 52 weeks, of 7 days each, and years of 365·25 days, the decimal divisions of time had to be abandoned; and at this point I am led to the main object of this paper—namely, to discuss the new "Libric system," proposed and brought under our notice by its author, Mr. J. R. Napier, besides referring to other points in that gentleman's paper. It is quite clear that although Mr. Napier does not like the metre, he is certainly in favour of denary multiples and sub-multiples of his standard.

Now it would seem that a great feature, which Mr. Napier

considers ought to obtain in a metrical system, is that it should have what he has styled a "miabatic" quality, and this he has pointed out as the all-important quality, which the Decimal Association and others who have favoured the French metrical system have overlooked.

When their metre is the standard unit of length, ten metres square the standard unit of surface, the $\frac{\text{metre}}{10}$ or decimetre cubed the standard of capacity, and the 1,000th part of that capacity of distilled water or the gramme, at 4° C = 39°·4 Fahr., the standard unit of weight, how can the system be said to be *mia*batic? The fact is, that for each order of measurement a separate base or radix is employed. In short, it is impossible to have a complete metric system, "miabatic," for length is one thing wanting its own base; weight is another, depending on specific gravity; time another; and temperature another; coinage being still another: we cannot find a base common to all of these. And to give due expression to such terrible incompleteness, and that, too, without departing more than a step out of the road in which Mr. Napier has taken a licence to travel, I have, without strictly following his example of unnecessarily coining a new adjective from a Greek root—for Mr. Napier seems to have forgotten that the chemists have an adjective which exactly expresses what he has sought after, namely, "monobasic,"* which I think, too, the chemists would have lent him the free use of—thought it better, as stating the conditions more accurately, to adopt also from the chemists their adjective, "polybatic" or "polybasic," hence signifying the "French metrical" and the "Libric" as the many-base systems.

Again, in order to shew some special advantage attaching to the French metrical system, Mr. Napier says,—"French plumbers can know the number of litres a tank will hold by the simple product of the length, breadth, and depth; whereas a British plumber, before he can know the number of gallons his will hold, has to multiply the product of the dimensions by 6·23 or approximately

* Whilst I make this suggestion, I do not of course mean to imply that the adjective, "monobasic," has quite this same meaning to chemists, which it is well known is not the case, but, then, the Greek roots are identical; and I merely draw attention to the reprehensible tendency of the age to still more encumber our present over-crowded language, by coining new words, when in reality, if we look for them, it will in almost all cases be found that words already exist which will exactly express what is sought after. It is well-known how elastic is the meaning of a word!—St. J. V. D.

by 6¼." But, I would remark, that whilst it is undeniable, if a nation's measurements were limited to converting capacities into standard water-weight equivalents, the metrical system is by one arithmetical step simpler than the British, and in so far does offer perhaps, on a first view, some slight advantage to a shipbuilder when knowing the weight of materials in his ship, he desires to convert that roughly into displacement expressed in capacity. Yet remembering that sea-water is to pure water at the standard temperature, 39°·4 Fahr., as 1·026 to 1·000, it is obvious that he cannot make even that conversion without recourse to a table of specific gravities; nor do I see, except for the plumbers themselves, who at most constitute but a very small fractional part of a nation, that the inevitable step of using the specific gravity coefficient, and therefore the arithmetical process under which that is employed, can be dispensed with. It is surely one-sided to limit our view of the subject in this restricted manner; nay, the very narrowness of the view argues powerfully against the "Libric" system being even considered, to say nothing of being adopted here in Britain. For if we are to tie ourselves to limits whose higher expression is water, whose lower expression is also water, we may in all due wonder enquire, What does the Frenchman do when he wants to get the weight of a measured bulk of iron, or stone, or wood, or grain, or indeed anything else whatever than water?

In another place, too, Mr. Napier says,—"For shipowners, grade callipers would give the weight of any package, as water, and save them labour in estimating what the freight ought to be." The former part of this passage may be sufficiently near the truth, but I fail to see how the latter part is so; and on the *reductio ad absurdum* principle, I ask whether British merchants export and import water only? How then are the grade callipers to save labour in estimating freight when the materials to be estimated upon are always something other than water, and therefore of different specific gravity?

In telling us of the proceedings of the French when they determined to abandon their old measures, that they commenced by fixing the length of their metre, and from that got their litre and kilogramme, Mr. Napier omits to point out the blunder they fell into in making their unit of weight so ridiculously small that it had to be multiplied by 1,000 before a quantity sensibly appreciable, and therefore useful to ordinary people, and at the same time fitting in with the character of their denary integer-multiple scale was arrived at. And yet, what a notable fact in these times it is, that

after an International Metric Commission has met in Paris in four successive years, and has arranged a programme for its future operations for constructing and maintaining there, new prototypes of French Metric weights and measures, and a part of whose duty is to make as required or deemed fit comparisons of international standards—that one of the European nations, long under the yoke of a power once living, but now dying, nay, almost dead, should withdraw from that commission; and as Professor Quirico Philopante, and others, have recently shewn, that although King Victor Emmanuel's government have sanctioned the introduction into Italy of the French metric system; yet, in Italy, a reaction is going on in favour of the maintenance of the Roman ℔, foot and amphora, a system which, in respect of its linear and cubic relations, varies but infinitesimally from the linear foot, troy pound, and cubic foot of our present British system.* It is, however, no less notable that Mr. Napier has helped us, too, in the direction of Italian choice, for his proposed new linear standard—the gradus, grade, gressus, or gress—is, he tells us, 2·5 Roman feet.

Mr. Napier proceeds to say that we, however, "having already a pound and gallon, neither of which we wish to alter if we can avoid it, have to find a length which shall have metric qualities." Indeed, I think we cannot too greatly appreciate Mr. Napier's practical wisdom in thus pointing out, near the commencement of propounding a new scheme, that we British folk possess certain quantities and relations among them in our standard measures which are of too much importance for us easily to throw aside for some alleged advantages of the French metric system; but then he does not go far enough, for he says nothing of the acre, which to the landed proprietors and land-rent payers is of the utmost importance, seeing that all the land of this country is specified in title-deeds, leases, and so on, in terms of that acre; and whilst passing over intermediate standard quantities, I may refer to the extremes, namely, the inch and the mile, both of which are so deeply engrained in our nation, from numerous viewpoints, too, that those who have been entrusted by Government to

	British Inches.	British Foot.
* Old Roman foot	= 11·64	= 0·97
Old Roman amphora	= Roman foot cubed.	
	Lb. Avoirdupois.	Lb. Troy.
Old Roman pound weight	= 0·71	= 0·946

The Romans kept the standard amphora at the Capitolium, and the amphoræ in common use which corresponded with it were consequently called "Amphoræ Capitolinæ."—St. J. V. D.

consider the metric question have felt the impossibility of setting them aside. Mr Napier, in terms of the denary relation of the pound and gallon, proposes to multiply by 10 the cube root of the volume in water of our standard lb, and to erect that into a standard length-unit, viz.:—$10\sqrt[3]{27{\cdot}7274} = 30{\cdot}267$ British inches; but then he does not tell us at what temperature his standard length is taken! In fact, whilst denying the French metric system as totally unfit for Britons, he copies it in its weak parts, by looking to water as the only physical substance with which man has to deal, dividing everything by 10, without considering its complete fitness or not; and yet neglects altogether a standard temperature, and above all, the fitness of a standard of length for all purposes, both commonplace and scientific.

It seems to me, further, that in dealing with this question we must not consider only that which is most expedient and cheapest for the present time merely,—and this is the view to which Mr. Napier unfortunately addresses his attention, in order to avoid the vast expense of a wholesale revolution in our national standards,—but that, as in most other questions of high political and scientific import, we are compelled to look far ahead in order to provide as much as possible a system which shall meet the requirements of the distant future with equal fitness to the immediate present. Happily, then, our law-makers appear to have taken this view, for we may lean with a perfect trust on the recent announcement of the Astronomer-Royal in his late annual address to the Royal Society, wherein he spoke as follows:—"I think it imperative on me to state that the British Government gave their assent (to the establishment of an International Metric Commission) only on the express understanding that they could take no part in the Commission if it displayed any propagandist intention. Speaking as the representative of the body who had best considered this subject, namely, the Standards Commission, now dormant, I can say as their unanimous opinion, that they deprecate the slightest interference with national usages." And surely not only should this very sound and sensible conclusion of that Commission be prominently held in view by every one dealing with proposed alterations in our metrology, but such persons should take heed to the experience in France of nearly a century, which is the time it has taken to introduce the metric system there, to say nothing of the vast confusion and waste of time that have accrued all over the world thereby, to those who have been taught to buy and sell, each, on their own national systems.

Mr. Napier next goes on to say that the divisions of this new standard may be called decigrade, centigrade, milligrade, and in so far still copying the French metric system in its most anti-British and anti-Saxon portions. He then goes off into quinary divisions, which he elsewhere decries, by telling us that the centigrade divisions may be further subdivided into *five* parts.

Here we see, then, that as a necessity, on ground of alleged convenience, Mr. Napier resorts to quinary divisions, although he elsewhere deprecates a quinary scale.

Again, he alludes to the recently introduced $\frac{1}{4}$ gill, and to the imperial bottle and half bottle, of which 6 and 12 respectively go to make the gallon; and here I think, with him, that when adding further subdivisions to our already so thoroughly engrafted denary pound and gallon, it would have been better to have legalized denary bottles of 5 and 10 to the gallon respectively. That, at least, would have restored to us the good old feature of former days, when in some cases weight and capacity units were alternative equivalent expressions, which our law-makers with lamentable unwisdom set aside, namely, that,

"A pint's a pound
All the world round."

and with much confusion, by reason of their change having introduced fractions into our simplest calculations, where conversion of weight into capacity, or *vice versa*, is involved, so that the pound unit and pint unit are no longer alternative expressions for the same thing, the former being the tenth and the latter the eighth part of the gallon, as the other well-known couplet reminds us that,

"A pint of pure water
Weighs a pound and a quarter."

It is said the grade is as good as any other stick for measuring a distance, and whilst that may be the case for some distances, it is not altogether true. For example, it is not as good as a measure founded on Earth's radius or diameter for measuring and expressing astronomical distances, which are naturally, nay inevitably measured, and very often expressed too, as I have already said, in radii or diameters of the Earth-globe itself, or in radii or diameters of its orbit.

Whilst Dr. Joule, to whom we are all so much indebted, says in his letter which Mr. Napier prints, "it is undesirable to make a new scale unless there should be an absolute

necessity for it. My idea about the metric system is that it has always been a mistake, not on account of its defects, but because no power of government will ever force the British workman to give up his 2-foot rule." Now this is indeed a sound piece of practical truth from Dr. Joule, and although applicable to the whole question of metrical changes, is very suitable as coming from one who has had so much to do with the building up of our heat knowledge into one of the best perfected branches of physical science. Such an one as he, surely, should feel the desirability of a change in our thermometrical scales, if any such change is desirable or necessary. And whilst we cannot but admire, from its elegance and completeness, in the abstract, Mr. Herapath's thermometric scale—which Mr. Napier has also quite originally and independently worked out—yet, ere bidding farewell to our present scales, we must not forget that the new thermometric one is based at best on a theoretical deduction which no man ever has, ever can, or ever will see.

Whilst, then, I have found myself unable to coincide with a great many details of the subject which we are indebted to Mr. Napier for laying before us, yet I must, before closing my remarks on his proposals, say, that at one part of his paper he has expressed what appears to be so completely reliable, that I cannot forbear quoting the passage, thus:—"At present it happens to be the British ton of 2,240 lbs. which rules in the Mediterranean ports; and we have an idea that the British engineer and British merchant will compel British measures to be accepted long before the French system." We should have been glad if the author of the paper had also informed us in how many ports in other seas British weights do also prevail; and we think that those in the Mediterranean would form but a small fraction of the whole. It would have been useful, too, if he could have told us what proportion of the civilized population of the whole world still use British weights and measures in preference to the French metric system, and I think he would have shewn that to be by far the larger portion.

Mr. Napier has referred to what he has been pleased to call Professor Smyth's Pyramid system, which he, and I think quite rightly, considers far better than the British system as it at present stands. He also says that this system is the only modern one which aims at being international; and whilst its merits have not been discussed, he very rightly thinks they ought to be before any one condemns it, as so many have presumed to do without having given it candid consideration.

It is not my intention to fully discuss that system this evening, and it is now too late to commence to do so; but I merely wish to correct some mis-statements which Mr. Napier has, I think, quite unwittingly made respecting some of its features.

In the first place, the system is not of Professor Smyth's origination, although a great many persons who ought to know better assume it to be so. The first view of the system was published to the world by its discoverer, the late John Taylor, in 1859; and many of the salient points were so convincing to the late Sir John Herschel, that he hesitated not to adopt them, and strenuously advocate them in public as the very best the British nation could adopt—in short, a system with which several of our present weights and measures almost so exactly coincided, whilst being founded on the most international, the least alterable, the best measurable, and altogether the purest line in nature, the aforesaid Earth's axis of rotation, and this years before our own Astronomer-Royal had ever considered the subject. What Professor Smyth did was this, to submit John Taylor's conclusions (backed up, as they were to a great extent, by Sir John Herschel) to a most searching examination, in order to test the possibility of there being any soundness in them; and finding that no satisfaction could be got with any sufficient approach to precision from the literature of the subject,—although the Pyramid is, perhaps, the one monument of ancient days which has been examined, illustrated, and written about more than any other,—he determined on testing the whole of Mr. Taylor's theory in the one only possible way to eliminate the truth respecting it, undertook, at his own cost, to visit Egypt and carry out, on the Pyramid itself, the most exhaustive and intensely exact set of measurements, conducted with the most refined methods which modern science has yet invented for eliminating errors, whether personal or real, which have ever been applied to any building, ancient or modern.

Now, the fault which Mr. Napier finds with that system is this, that it is not "miabatic," but, as aforesaid, he has yet to shew us that any complete system of metrology ever was or ever can have that quality; and unless we do include all branches of measurement into our considerations, it seems to me idle to touch the matter.

It is perhaps scarcely necessary to remind you that in the East the belief has ever been, that one purpose for which the Great Pyramid was built, was to enshrine therein some primeval system of weights and measures; that numerous investigations have been conducted by some of the foremost minds in Europe to find out

that system; and that no one worked at it more assiduously, and at the period so successfully, as that greatest of British philosophers, Sir Isaac Newton; whose work on cubits will ever constitute a link of first importance in the chain of evidence leading up to a solution of the great metrological question. So firmly grounded was Newton's mind, in the record coming down to him grey with age, that he concludes his investigation in words which sound with an almost prophetic import, in view of what has since happened. Newton says,—"Those who shall hereafter examine the Pyramid, by measuring and comparing together with great accuracy more dimensions of the stones in it, will be able to determine with greater exactness the true measure of the Cubit of Memphis, and from thence likewise the Sacred Cubit." What a very singular fact then is it, that with testimony of no other than Newton, nearly two centuries ago, John Taylor, Sir John Herschel, Piazzi Smyth, and the elder and younger Petrie, should, from other conclusions, based on more refined measures, of that very Pyramid itself, shew to all men not merely the verification of those words of Newton's which I have just quoted, but also shew forth what Newton never dreamt of—viz., that not only were those notions of his in respect of the Pyramid true, but that the unit of linear measure there, is no other than an evenly commensurable part of the Earth's polar axis, no other than our present British unit increased by one thousandth of its own length!

Surely, then, with the evidence I have shewn that a perfect metrological system should be commensurable with the Earth's polar axis, in order to suit not merely terrestrial but celestial or astronomical measurings and weighings also, and finding that our existing system is so very nearly in accordance with such requisitions, whereas neither the French "Metrical," nor the "Libric," system is so, we should pause ere we plunge into any other system which is either absolutely a British novelty or absolutely French.

Mr. Napier has shewn us, as communicated to him by Sir Henry Rawlinson respecting the primitive Babylonian system, that it was sexagesimal, that is to say, founded on the most extensive scale, in respect of the great number of equal divisors which 60 contains; after which it gave place to a decimal system. But then, let us go a step further towards the beginning of time, into the older period of Proto-Egypt, I say "older," for in the matter of dates, it would seem that, however learned and accurate in other respects, Sir Henry Rawlinson is completely at sea, and we find

there, not a sexagesimal scale, but one on a quinary base; and this brings about a question which I think has not yet been properly weighed with respect to whether, for a national or international system of weights and measures, it is not everyway better that such a system should be founded on the simplest of all scales, that is to say, on one whose integer radix has fewest even divisors; for, indeed, the French Metric system which has 10 as its base, this number having four divisors, is used in that system quite irrespective of any other divisors than 10, and 1, 2, and 5 are never referred to. If, however, it be advantageous to have a powerful scale, then our present British duodenary one is far more powerful than the French metric denary one in the ratio of 6 to 4.

Yet, is it not better, as being more simple for the ill-educated and less liable to produce error for the intricate computer to have a base with fewest divisors, which we may get by using some of the digits as a base? All of these except 5 would be awkward in the extreme; 5 has all the conveniences which 10 has for a base, whilst its introduction would involve no sensible alteration in our present linear, surface, and cubic measures: not only so, it further seems to be the foundation of that international system which appears at one time to have prevailed, namely, the inch, pound, and ell—the record of so many ancient nations containing these. Now the ell varies from 2 to 2·5 feet in various countries, that is to say, it is sometimes found larger, sometimes smaller than what Sir John Herschel called a cubic of 25 geometrical inches, and which Sir Isaac Newton, John Taylor, and Piazzi Smyth have shown to be the cubit of the Pyramid. Curiously enough Mr. Napier's grade corresponds almost exactly with the Austrian ell. But 5 expresses several features which neither 10 nor any other number can express. For we have 5 inches of matter of Earth's mean density for our weight unit, or pound, equal to 28·5285 cubic inches, if expressed in distilled water at 68° Fahr. and barometer = 30 inches = 1·029 lbs. Avoirdupois = also to 1·029 old pint measure. Dividing that again by 5, we get the cubic inch, which may also be a weight and capacity measure. Again, we have the square of 5, or 25 inches of matter of Earth's mean density = 5 lbs., a 5 inch cube = 25 lbs., or referred to the 25 inch linear standard, we have a 25 inch square an inch thick equal to 125 pounds; 4 inches thick, 500 lbs. and so on, so that we merely have to know the capacity of a body in terms of that standard, and we get instantly its weight equivalent in terms of Earth's mean density, which, by the aid of a table of specific gravities we can easily convert into the equivalent for any other substance

we please. Not only, then, as extremely convenient for common purposes and incurring but an infinitesimal, and therefore practically inappreciable change from what we have at present, can we restore the use of the earliest system of metrology known. But what is more, being, in so far as weight is concerned, founded on Earth's specific gravity, it is admirably suited for astronomical purposes also; for it is the practice, and has long been so, to speak of the densities of the other kosmical bodies in terms of Earth's mean density. Whilst then I have merely referred specially to length, capacity, and weights, it is only necessary to add that for surface, temperature, and angular measures, the system is equally convenient, and in these respects applicable to our present metrical language and quantities without sensible change, and that it is the only scheme yet proposed which seems to meet all the requirements of a useful system of metrology.

Notes added by MR. DAY, *on 10th February, 1873.*

Note A.

In case it may be enquired how the quinary system is worked in the four common arithmetical processes, I have thought it well to give examples.

(1.) *Addition.*

Add together 91 cubits, 20·7 inches; 57 cubits, 18·29 inches; 74 cubits, 23·4 inches; and 16 cubits, 17·9 inches.

We have then—

c. i. c. i. c. i. c. i. c. i.
91, 20·7 + 57, 18·29 + 74, 23·4 + 16, 17·9 = 238, 80·29.

When, at a glance, we see that the cubit of 25 inches is contained in 80·29 3 times with 5·29 as a remainder. Add the 3 cubits, and we immediately write down the result as—

241 cubits, 5·29 inches;

and if it is required to express 5·29 inches in decimals of a cubit, multiply it by 4, shifting the decimal point two places, and we get

5·29 inches × 4 = 0·2116 cubits

as the result.

(2.) *Multiplication.*

Multiply together 943 cubits 17 inches, 761 cubits 19·8 inches, 374 cubits 24·9 inches. In a moment then we write down the figures in computing order, performing the same operation with the inches and decimals of the inch, which we saw done in the

preceding example of addition, namely, dividing by 4 and shifting the decimal point two places, and we have

$$943\overset{c.}{\cdot}68 \times 761\overset{c.}{\cdot}792 \times 374\overset{c.}{\cdot}996;$$

that is to say, we bring the figures into the same arithmetical state as metres and centimetres, or that of Mr. Napier's grades and centigrades. And although there may be some little trouble in the preliminary multiplying by 4, yet that is not greater than with the operation we must perform in converting inches into decimals of a foot or yard before we can conveniently multiply with them in our existing arithmetic. Further, for *addition* the advantage which this quinary system has over any one involving units with hundredths divisions is very great, for with it we never have any large numbers to deal with, such as 70, 80, 90, &c., which it is well known in tall columns of addition become formidable to even expert computers.

From the preceding examples of Addition and Multiplication the converse operations of Subtraction and Division will be obvious.

Note B.

As I have referred somewhat prominently to the ells of several countries as varying on the + or − side of a standard of 25·025 British inches, I have thought it useful to append a list of the several values of that standard I have up to the present time been able to collect. This list will be useful for reference.

		British Inches.
Earth's polar radius, $\frac{}{10,000}$		= 25·025
Cubit of Great Pyramid		= 25·025
Scale of Ordnance Map of Great Britain,		= 25·344 to the mile.
Denmark,	Ell	= 24·71
Austria,	Elle	= 30·66 = Napier's Grade + 0·393 B.I.
Prussia,	Elle	= 26·258 to 26·260
Sweden,	Aler	= 23·38
Norway,	Aler	= 23·38
Russia,	Archine	= 28·00
Turkey,	Pik	= 26·80 to 27·00
Bavaria,	Elle	= 32·79
Saxony,	Elle	= 22·29
Bohemia,	Elle	= 23·20
Augsburg,	Elle	= 24·00
Nuremburg,	Elle	= 26·00
Bremen,	Elle	= 22·76
Brunswick,	Elle	= 22·46
Frankfort,	Elle	= 21·54
Greece,	Picha	= 25·00
Candia,	Pic	= 25·11
Hamburg,	Elle	= 22·58
Hanover,	Elle	= 22·90
Lübeck,	Elle	= 22·90
Modena,	Braccio	= 24·52
Parma,	Braccio	= 21·34
Portugal,	Covado	= 24·4
Roman States,	Elle	= 25·041
Ancona,	Elle	= 25·33
Genoa,	Braccio	= 22·88
Turin,	Picha (?)	= 23·60
Florence, Leghorn,	Pic	= 22·98
Milan, Venice,	Pic	= 23·42
Stuttgart,	?	= 24·18
Berne,	Elle	= 21·40
Lucerne,	Elle	= 24·71
Zurich,	Elle	= 23·64
Geneva,	Elle	= 23·03
Bâsle,	Elle	= 21·40
St. Galen,	Elle	= 24·20
Island of Java,	Elle	= 27·75
Morocco,	Pic	= 26·00
Tripoli,	Pic	= 26·42

SIR WILLIAM THOMSON said:—When I pointed out that the metre had the advantage of being a convenient sub-multiple of the earth's quadrant, and claimed that this advantage was very much greater than that which Sir John Herschel pointed out for the British inch in being a sub-multiple of the earth's radius, I did not mean to imply—indeed, I think I said the contrary—that I approved at all of the system of adopting either the quadrant or the radius as the standard of reference. It is quite a mistake to say that the earth is the most constant body that we have. Witness the raising of one-half of Sweden; witness the geological evidence we have as to the raising of the bed of the ocean. If radii vary, quadrants can not claim to be very constant. The fact is, that even a base on the surface of the earth is liable to variation. What portion of the surface of Scotland is not liable to variation? The most accurate and experienced geologists hold that faults are going on just now giving rise to very considerable variations of the surface of the earth. Now, I say that Mr. Napier's standard, or the French metre, or the British standard, will thus time a hundred million years to be much nearer the same as now than any one quadrant of the earth's surface that may be fixed on. I cannot fix the precise degree of accuracy that has been attained in measuring the earth's polar axis. We have the interesting statement which Mr. Day has given us from Sir John Herschel. But such a comparison as this is certainly less accurate than the comparison of the different rods used in measuring the earth. It is not possible that the measurement of the earth's axis, or any quadrant of the earth, can be as accurate as the comparison between the national standards of length. It is simply an error to say that the metre is "the ten-millionth of the earth's quadrant." The metre is anything you have which is as long as the one normal standard metre—that one of the individual standard measures in Paris which has been declared to be *the metre*. That is the metre. You may take copies of it accurately, but you can never find accurately the length of even any one of the earth's quadrants. The comparing of the British yard with the French metre, and the comparing of the British pound with the French kilogramme are, I believe, the most accurate investigations ever made in science. The comparison by Professor Miller, of Cambridge, of the different standards of mass of different places was made with marvelous accuracy. What I say in regard to the advantage of the metre being approximately enough for all practical purposes, a ten-thousandth of the earth's quadrant, instead of any round

sub-multiple of the earth's radius, and about our not making journeys into the centre, is that it is exceedingly useful for practical purposes to have convenient factors, such as ·0001, ·09, ·54, for reducing from kilometres to quadrants, or to degrees, or to minutes, in estimating distances on the earth's surface. I say that is a real practical advantage; and I say emphatically the inch has no practical advantage whatever, from being approximately a five-hundred-millionth of the earth's radius.

Mr. NAPIER, in reply to those who had spoken and written regarding the paper he had read on 8th January, stated that he did not propose the grade as a permanent British measure, but as a temporary one, to satisfy those who called for a uniform decimal system, and those who desired the connected quality of the French system. He was himself indifferent as to what system or base was adopted in Britain or in England, if that term be taken to mean the lands governed by British laws, provided that it was arrived at after the most talented and scientific men requested by their country to consider the matter shall have reported their views. Till then, he submitted that Sir John Herschel's foot decimally divided, the grade, and the two other similarly derived measures which he had proposed, were each and all as good and efficient as the metre, and could be introduced at two-thirds of the cost or trouble of introducing the metric system. His chief objection to that system was that which Sir Wm. Thomson had himself admitted, that the founders of it did not take the nautical mile as their standard or base; for while sailors and geographers were left to make miles for themselves differing from the statute mile, the system which allowed it could never become universal. And as Sir Wm. Thomson had given his opinion that mathematics is too strong to permit the French even to make their sea mile 1000 metres, and as the facts are well known to be as Mr. Garbett has stated in his letter and paper, it appears that Sir Wm. has virtually settled the question against himself and the metric system. With respect to the two or three-foot rule being ingrained in the minds of British workmen, he (Mr. Napier) was satisfied that in a shipbuilding yard, employing say 2,000 workmen, there would not be the least difficulty in getting the men to use the rule he proposed or any other. If it was meant that there would be difficulty in designing with the new rule, that was another matter; but of these 2,000 men probably not twenty ever employed a rule for that purpose, and he was convinced that the men who

would require it for designing would have no difficulty in learning its use. Certainly not more than in designing with the metre. They could learn to design with it or any other rule quite as easily as Sir William himself has learned the use of the metre. As to Mr. Day's objection that his views were not large enough, he (Mr. Napier) was sorry to know that that statement was too true. He had done his best, however, by shewing his ignorance to have his views enlarged: he would assuredly have written less had he known more. Mr. Day's objection or statement that the smallest divisions marked on the grade rule was fifths of the next larger division instead of tenths was, unfortunately either for Mr. Day or himself, the only part of the rule that he considered there was any real invention in. To have left the larger interval blank was virtually to have consigned it to the binary scale, to be halved and quartered; to the halving there was no objection, but to the quartering there was, to all pure decimalists; therefore the division into five parts. But this does not in any sense make the rule quinary, even if there were no figures marked on it, and with the figures so plainly marked in the denary scale he thinks it unlikely that any one would mistake in reading it more than they would in reading the $\frac{1}{4}$ $\frac{1}{8}$ or $\frac{1}{16}$-inch in the duodecimal foot-rule. The result of the Admiralty definition of the "British Sea Mile" given in the paper is, that it is about 6 feet longer at Wemyss Bay on the Clyde than at Stokes Bay, and that the Wemyss Bay "nautical mile" is about 8 feet shorter than the "British Sea Mile" there ought to be.

XVI.—*On Numeral and Metral Systems.*—By MR. E. L. GARBETT. *Communicated by* MR. JAMES R. NAPIER.

Read before the Society, February 19, 1873.

Some of the properties of different scales of arithmetical notation are pointed out in several books of secondary instruction, intermediate between commercial ciphering and algebra, especially that of Mr. Sang; but beyond popular and somewhat vague reasons why a senary or duodecimal one would have been preferable to the denary, and no other small base would equal its utility, any distinct propositions like the following, comparing relative values of all scales, I have not met with:—

PROPOSITION 1.—*If binary and ternary arithmetics were in use, the binary would be the more useful in the ratio of* $\frac{1}{\log. 2}$ *to* $\frac{1}{\log. 3}$, *or about* 477 *to* 301.

For all scales are of equal use in notating integers, or all the multiples of every unit they can write. Therefore their relative values depend only on the more or fewer *quotients* they can express by finite figures—that is, on their power to express mixed numbers or fractions in one line. But it is shewn by all who treat on scales of numeration, that a binary scale could express no fraction whose denominator is not a power of 2; nor could the ternary write any whose denominators are not powers of 3. Now, confining our attention to fractions whose numerator is 1, all these might be represented by points on a logarithmic scale (or slide-rule) of infinite length in one direction, from 0, the log. of 1, to negative infinity, the log. of 0; and their multiples, that is, quotients of other integers above unity, would be a series running from each point back towards the 0 of the scale and beyond it, and being equally numerous for each denominator, may be neglected. The points representing powers of $\frac{1}{2}$ would be ranged at equal intervals, and those representing powers of $\frac{1}{3}$ would be another series equidistant in themselves, but at a wider interval than the former, as log. 3 exceeds log 2. Hence there would be fewer of them in any given range than of the former points, as $\frac{1}{\log. 3}$ to $\frac{1}{\log. 2}$. Hence the variety of broken quotients expressible by binary notation exceeds that expressible in the ternary (though both are infinitely numerous), in the finite ratio of 477 to 301; and in this ratio would the binary excel in utility the ternary.

Corollary 1. Hence it follows that *quaternary* or *octonary* scales could notate no quantity that the binary does not; nor could the *nonary* express anything that the ternary does not; nor any scale whose base is an exact power of a number notate any quantity more than a scale based on the said root number.

Cor. 2. Hence also we see that, of all notations whose scale is a *prime* number, the *binary* must be the best, and its notative value exceeds that of any with a higher prime base, in the inverse ratio of the logarithms of 2 and that base. Thus it excels the quinary (or vigintiquinary), as $\frac{1}{\log. 2}$ to $\frac{1}{\log. 5}$, or as 699 to 301 nearly.

PROP. 2.—*Though the relative values of the binary, ternary, quater-*

nary, and quinary scales can thus be numerally compared, the advantage of the senary over all these is not expressible by any ratio, but is relatively to them infinite. For, besides expressing all that the binary and ternary could separately, that is, all quotients by the powers of 2 and by those of 3, it could plainly give all intermultiples of two quantities belonging to both these classes—*i.e.*, products of any power of 2, positive or negative, by any power, positive or negative, of 3. Now, recurring to the logarithmic scale on which we supposed two series of points marked for the powers of $\frac{1}{2}$ and of $\frac{1}{3}$, and calling the uniform interval of the former a, and of the latter b; to represent this third class of quantities, we must set off from every point of the a series, a series in each direction with intervals $= b$; or else from every point in the original b series, a series each way with intervals $= a$. Now, none of the points thus marked can fall on any of the old points, because a and b are incommensurable. And such double new series has to be marked off each way from every point of the old series a, or else of the old series b. Hence, by this process the number of marked points must be multiplied *infinitely*, not only in the infinite scale, but also in any finite range. That is to say, out of the three kinds of fractional quantities that the senary notation can express, namely,—

(Class A.) Those whose denominators are powers of 2;
(„ B.) Those whose denominators are powers of 3, and
(„ C.) All intermultiples of both these, or $2^{\pm m}3^{\pm n}$.

the latter class infinitely outnumbers A and B, either in any finite range, or on the infinite scale. For though on the latter, each class of the three is infinitely numerous, A and B are only infinites of the first order (bearing to each other the finite ratio of about 301 to 477, as above seen), but C is an infinite of a second order. Hence, in comparing the notative powers of, say, the binary, ternary, and senary systems, the ratio of utility between the two former vanishes in presence of the latter, its value exceeding both theirs infinitely.

Cor. 1. This advantage of the number 6 arises, we see, simply from its being the lowest that has *two prime divisors*, and hence the next that has two such divisors, namely 10, or any higher that divides by two primes, and by only two (say 12 or 15), would have the same infinity of advantage, in notation, over any that has but one prime factor, as 8, 9, 16, 25, 27, 32.

Cor. 2. Moreover, it appears that multiplying by any prime which the base already contains as a factor (as multiplying 6 by 2)

can add no notative power; as we saw from *Prop.* 1 that exact powers, used as bases of notation, can enable nothing to be expressed that their lowest root could not. Thus, as quaternary notation can write no quantity that binary could not, so the *duodenary* has no additional power to the *senary*. For the prime factors of 6 or of 12 are equally 2 and 3 only. Hence duodecimals and the senary scale have exactly identical power. Whatever either can notate, the other can, and nothing else.

PROP. 3.—*Though the senary notation and our common denary one both excel infinitely* (as above shewn) *any whose base, except 6, is less than 10, their utilities may be compared among themselves; and the senary is thus found superior to the decimal as* $\frac{1}{\log. 3}$ *to* $\frac{1}{\log. 5}$, *or as* 699 *to* 477 *nearly.*

For we have seen that the quantities expressible by senary notation are of three classes,—those denominated by powers of 2, by powers of 3, and the intermultiples of both; and the two former classes vanish in any comparison with the multitude of the latter. Similarly the quantities expressible decimally are of three kinds,—those denominated by powers of 2, by powers of 5, and quantities that are products of both these; and this third class infinitely outnumbering the two former. To compare the senary and denary systems, then, we have only to compare the *third class* of the quantities that each can notate, because the ratio of the two other classes to the third is, in each case, evanescent. Now, returning to the analogy of the infinitely long logarithmic scale, the number of points thereon representing powers of 2, in any finite range, is finite; as also those representing powers of 3. But from each of the former points are to be set off a series each way, with the constant interval of the latter series, or a; and this being done from an infinite number of points in the whole scale, it renders the third class of *senary* points infinitely numerous, even within a finite range. Next, to represent the third class of *decimal* quantities, we must, from the same points as before, namely, those representing powers of 2, set off series each way, but with a constant interval representing log. 5, and therefore exceeding the former interval a which represented log. 3. Hence the whole infinite multitude of *this* third class of points, in any given range, will be less numerous than the multitude of the *other* third class that represented senary quantities, in the ratio that $\frac{1}{\log. 5}$ is less than $\frac{1}{\log. 3}$. For though both of these, the third class of *decimal* quantities and the

third class of *senary* ones, infinitely outnumber their first and second classes, yet they do so by two infinities of the same order, and bearing to each other this finite ratio, about 477 to 699. And in this ratio, therefore, does the whole notative value of the *senary* or *duodecimal* systems (these being precisely equivalent) excel that of the common or *decimal.*

Cor. Hence it appears that, of all possible scales whose bases may be a product of two prime numbers, the senary and its multiples are the best, and denary the second best; for the larger of any numbers must have the larger logarithm, and the smaller reciprocal thereto. And as this fact makes all scales with prime bases above 2 inferior to the binary, and of less value the higher their base, so must it reduce the notative worth of those that contain two prime factors, the higher these are; from 6, the first bi-primal number, upwards.

PROP. 4.—*As infinitely as the senary and denary scales have thus been found to excel in power the lower ones, just so may a class, of which the trigintal is the lowest, be proved to excel the senary.*

If it were desired to have a scale notating every fractional quantity that either the senary or decimal scales can express, it is obvious that the lowest base answering this purpose would be the product of all the three primes contained in both, that is, $2 \times 3 \times 5 = 30$. A trigintal scale, then, has their combined powers, or notates all these five classes of quantities that alone the senary and denary could between them.

A. Those whose denominators are powers of 2.
B. Those whose denominators are powers of 3.
C. Those whose denominators are powers of 5.
D. All intermultiples of $2^m\ 3^n$ } m and n being taken.
E. All intermultiples of $2^m\ 5^n$ } either + or -.

But, besides these, it plainly introduces two new classes, namely,—

F, all intermultiples of $3^m\ 5^n$ and
G, all intermultiples of $2^m\ 3^n\ 5^p$.

Now, this last class, G, may be shewn to out-number *infinitely* the classes D, E, F. For, recurring to our supposed logarithmically divided line, and supposing the first six classes all marked by points thereon—if we are to mark points for the G class of quantities, we have to take those of either the D, E, or F class, and set off

each way a new series at intervals corresponding to Log. 5; if we have taken the D class, or if the E class, to Log. 3; or if the F class, to Log. 2. By either process, the same new points will result; and because no power of one prime number is equal to any power or multiple of other prime numbers, the common interval of the new series of points, however multiplied, can never coincide with a sum of any of the old intervals, and thus none of these points can fall upon a point previously marked. But from each of an infinite series, either D, E, or F, a new infinite series each way is to be set off. Hence, the new points G must (whether in the infinite line or any finite one), infinitely outnumber each and all of these classes, D, E, F. Trigintal numeration, then, has notative powers *infinitely* excelling the senary, or any by a base lower than 30; this incomparableness arising from the fact of 30 being the lowest number with more than two prime factors; as 6 is the lowest having more than one.

Cor. 1.—Hence we see that for numeral and metral purposes, different numeral bases can have their values compared and denoted by finite ratios, only when they have an equal number of prime factors. Thus, we have compared 2, 3, 4, and 5, which have each but one prime factor, and the comparison might have extended to 7, 8, 9, and all others of this poorest class, consisting of the primes and their powers. We have also compared with each other, but not with these former, the values of 6 and 10, because both are of the next poorest class, infinitely excelling the first, and which may be called *bi-primal*, as containing two prime divisors. We may now as easily compare the first few *ter-primal* ones among themselves, remembering each must infinitely excel all the bi-primal. The only numbers in the first hundred having more than two prime factors are 30, 42, 60, 70, 84, 90. Now, in 30 and its two multiples, the primes are the three lowest, 2, 3, and 5. In 42 and 84, they are 2, 3, and 7. Hence, these latter bases (which, like 6 and 12 as we have seen, must have identical value), are inferior in notative power to 30, 60, or 90, in the ratio of $\frac{1}{\log. 7}$ to $\frac{1}{\log. 5}$. Again, the primes in the remaining ter-primal, 70, are 2, 5, 7; so that it will be inferior to 42 or 84, in the inverse ratio of the logarithms of the two factors that differ in them; or, as $\frac{1}{\log. 5}$ to $\frac{1}{\log. 3}$, the same ratio in which denary or vicenary scales are inferior to senary or duodenary.

Cor. 2.—We see that there are an infinite progression of classes,

then, of metral value in numbers; and only those of each class are comparable among themselves. In the first or poorest class, 2 and its powers, which are the only *even* numbers therein, are all of precisely equal use, and excel, but only finitely, all odd numbers. The first even number that is not a power of 2, namely 6, heads the next or bi-primal class, and is superior to all that are not multiples of itself, as 10 or 20, while exactly as good as all its own multiples, except such as are multiples of 30 or some other ter-primal number. In like manner, 30 heads the next class, and excels, finitely or infinitely, all its non-multiples therein, and has no inferiority to its multiples till we reach 210, the first quater-primal. As the numbers in each class are better the lower the primes composing them, the first bi-primal, first ter-primal, &c., will each have the utmost notative power possible in its own class. Hence we get these heads of their successive orders, by multiplying into the first prime all the succeeding ones, 2, 3, 5, 7, 11 &c.; yielding this series 2, 6, 30, 210, 2310, 30030, 510510, &c.

PROP. 5.—*The very lowest in the class of numbers required to give any particular order of notative value, can never be divisible by any exact power, as* 4, *&c.* This is plain from the above mode of generation of these numbers. Each of them is double of an odd number, because all its factors after the first are odd, there being no other even prime than the number 2. As no single factor, moreover, enters into their composition twice, these products can divide by no square number, even 4; nor by any cube nor other power.

Cor.—Hence we see an obvious reason for not using in practical arithmetic any of these very first numbers of each class as a base, but rather the double of the first; as 12 instead of 6, and 60 instead of 30. This adds nothing indeed, as we have seen, to the absolute power of expressing quotients exactly; for all that can be notated in duodecimals could equally be so in the senary scale; but often only by more figures, and in some important, because very common cases, not without twice as many. This is the case of the powers of $\frac{1}{2}$, or negative powers of 2, that it is so natural to introduce in retail trade. Because 6 does not quarter, as 12 does, these descending bisections would require the same lengths of figures to express them in the senary that they do in the decimal scale. And so they would even in the trigintal; while in the duodecimal or the sexagintal they need but half as many. This is supposing the duodenary to have 11 characters, and the sexagintal 59, and the trigintal 29; but of course neither of the two latter ever has been or should be worked in this way. One main practical advantage

of both over duodecimals is their not demanding or even suggesting the use of any more than our familiar numerals. The trigintal (which I only mention as a curiosity) would be written indeed without the figures 6, 7, 8, or 9; and worked with no need of knowledge of the multiplication table above 25; which would be written "41," meaning "four sixes and one." We should carry sixes from the first and alternate places (that is, the units, *thirties*, 900*s*, &c.), which places would contain no figure above 5; while, from the intermediate or *even* places (which are those of *sixes*, 180*s*, 5400*s*, &c., and would admit no figure higher than 4), we should carry fives. The same alternation of course is necessary in the sexagintal scale (as actually used for the divisions of the circle and angles, and formerly for most other things)—but the two steps, instead of 6 and 5, are 10 and 6. Either a space or mark has to be made separating each pair of figures, or else the figures alternately distinguished in some way, as by two alternate sizes, to remind the computer of their different practical values; but in writing, if no such distinction were made, no error could thus arise if the sum be once notified to be sexagintal, even were the figures all alike and packed as close as in our denary notation. For the reader would only have either to point off, or imagine them pointed off, in pairs from the end, and read each pair, as he now does each triad, by the name of its denomination. Now, though, in trigintal notation, if ever used, the best method, as above described, would leave only the lowest 5 units to be expressed as we write them, six being written "1o," and thirty being written "10o," as I am only here about to use it once, for a comparison of the three systems, I will suppose its steps to be alternately 10 and 3, instead of 6 and 5, that each pair of figures may be read in our familiar way, being exactly as we write the first 29 numbers, only with the figure of tens larger than that of units. The sexagintal must of necessity notate up to 59 as we do (with the same modification only), and sixty is written "10o," being merely a compacter form of our astronomers' "1°oo′, or 1′oo," in the oldest of extant numerations, as widely spread as the denary, and that one which the French decimalists, in 80 years, have utterly failed to disturb, even in the Paris Institute. As the descending powers of $\frac{1}{2}$ would begin with more and more ciphers, I will subtract them from unity, and the increasing remainders, from $\frac{1}{2}$ up to 1, will appear in the three notations, thus—

Trigintal.	Sexagintal.	Decimal.
0·15	0·3	0·5
·2215	·45	·75
·260215	·523	·875
·28010215	·5615	·9375
·2900060215	·58073	·96875
·291508010215	·590345	·984375
·2922190016021 5	·59315 2,3	·992187,5

The trigintal would require double, but the sexagintal just the same number of places as the decimal, you see, for this very common class of fractions or quotients. This alone would be a sufficient reason for preferring the base 60 to 30, were we choosing a notation for the first time. But, when we find the sexagintal has been in use for ages, the oldest numeration according to Sir Henry Rawlinson, and actually remains familiar to the whole civilized world for certain subjects, divisions of hours, the circle and angles (though in an affected form needlessly cumbered with marks ° ′ ″ &c.), while the trigintal has never been in use that we are aware, and would, to all of us, be a new language, in its best form incomparably more puzzling and different from our familiar ciphering than the sexagintal (which is really *not* new wherever hours and minutes are used, or geometry taught), there can be no question about the change we ought to make, if any.

We have seen that any ter-primal scale-base excels in utility, not in a finite ratio, but infinitely, any bi-primal one whatever; and that 60 and 10 belong to these two classes; but while 60 is the very best of the ter-primal class, 10 is only of a second best order in its own incomparably poorer class; and not the best of even that order, for not only the whole order of 6 and its multiples are better, while only bi-primal, but so is 20, as our ancestors found in the case of the pound and shilling, for the same reason that the doubles of 6 and of 30 have everywhere been used, and not those unquarterable numbers themselves. Of course, in reverting to this most ancient but demonstrably best of continuous numerations, we should require short and plain names for the 60 and its powers, as mere arithmetical terms, like the present hundred, thousand, &c., apart from concrete units of any special table, as hour, degree, ton, mile, pound, &c.

Now it happens that the Chaldean names for the 60 and two higher denominations, as quoted by Greek authors, and identified in the cuneiform records by Sir H. Rawlinson, are monosyllables

that would quite as well naturalize themselves in English and most modern tongues, as in the pages of the French Antiquaries, who write them *sosse*, *nère*, and *sare*. The two latter seem to have meant only 10 and 60 soss, our 600 and 3,600; but we would shift them to the square and cube of a soss; that is 3,600, written 10o0o, to be a "nere," and 216,000, written 1,0o0o0o = a "sare." These three names would then express more briefly than our present denary ones, every number not exceeding eight sexagintal places, or up to our 12,960,000. For this unit, being the square of a *nere*, I would, on the analogy of "million," coin the derivative *nerion*. Thus we should need no compound like "a hundred thousand" till reaching the eleventh figure, equivalent to our 777,600,000, which we should have to call "a soss nerions." But the next step, being the square of a *sare*, may consistently take the single name, *sarrion*; and after the two steps of "soss sarrions," and "nere sarrions," coming to the cube of a *sare*, we may, on the the analogy of "milliard," call it a *sarriard*. After two similiar compounds, the soss and nere of this, as the next is a sarrion squared, it would, on the analogy of the million squared be a *bisarrion*. The names above billion, given in our present arithmetics, are really, as De Morgan remarked, purely theoretical; none having been yet applied, that I am aware, except trillions to the weight of the earth and planets in tons. But in the soss notation, we see that this form of nomenclature need not begin till above our trillion, therefore practically not at all; and to gain an idea of the powers and brevity of the nomenclature above proposed, we may observe that the following sexagintal number: 139,134430,302709,374639 would read thus—one-soss-and-39 sarriards, 13-nere-44-soss-and-30-sarrion, 30-soss-and-27-nerion, 9 sare, 37 nere, 46 soss and 39—but on our denary scale, 999,999 billions, 999,999 millions, 999,999. The former has but 45 syllables against 54 in the latter, and yet the six new words it uses supersede at least eight of ours, namely sixty, seventy, eighty, ninety, hundred, thousand, million, and billion. While the verbal numeration would thus be shorter, the notation takes slightly more figures, on the whole very exactly nine to every eight; because the numbers of them required to do the same work centesimally or sexagintally will be as the logarithms of 60 and 100, which are very closely as 8 to 9.

Senary arithmetic would employ, relatively to the common denary scale, more figures, as the logarithm of 10 to that of 6, or fully 9 to to every 7; and the binary scale, using only the figures 1 and 0, would take 100 figures to every 30 denary. Duodecimals, if worked

with two new characters for 10 and 11, would save barely 1 in 11 of the figure places; but as no such characters have ever been added, or likely to be, the figures have really to exceed, and the places for figures, full or empty, to be made double of those in our denary working.

With regard to the sexagintal nomenclature descending from an unit, instead of ascending, it seems strange that European astronomers and geometers have not, for the sixtieth of a degree or of anything, preferred, as Hindoos did, the Greek term *lept* to the more clumsy Latin "minute." Instead of second and third lepts, &c., of course the above terms would suggest the derivatives, *neret*, *saret*, *nerionet*, and *sarrionet*. All these units, and every multiple and sum of multiples of them, we must remember, express fractions of the commonest occurrence, that are interminable circulators in decimal notation. Moreover, as 60 is a ter-primal, and 12 and 10 only bi-primal numbers, the portion out of all possible fractions, that will terminate in the soss notation, outnumbers *infinitely* all those that are terminable in decimals or duodecimals. On the other hand, no quantity expressible in a terminate decimal can fail, in this notation, to terminate at some point between the number of its decimal places and the double thereof.

In computing by this scale, it is obvious that for addition, subtraction, and all multiplications or divisions by a single figure, the sole points the calculator has to remember are to place the big figures under the big, and little under the little, and to carry tens from the latter, as in our simple rules, but sixes from the sums or products of large figures. But in long multiplication or division, that is, wherever the multiplier or divisor has more than one figure, it is necessary to remember that multiplying by 10 or a large 1 alters the figures of the multiplicand, though multiplying by a small 1, wherever placed, (as 10o,10o0o &c.,) does not alter them; and accordingly every product of multiplying by the large figures 2, 3, 4, or 5, that is, by 2o, 3o, 4o, 5o, differs from the products by the corresponding small figures, 2, 3, 4, 5. Wherever the multiplier contains a large 1, it is easy to compute and set down, first the decuple of the multiplicand in its proper place, with its figure of tens under the said large 1; then to derive the products by each other large figure from the decuple, by doubling, tripling, quadrupling, or quintupling it; and after that, to derive the multiples by the small figures directly from the original multiplicand. In any case, one to whom the notation is new will be assisted by first setting down separately the decuple of the multiplicand, and then

multiplying alternately the simple multiplicand by each small figure of the multiplier, and this decuple by each large figure. In long division of any extent, it is usual and well to form first the several multiplies of the quotient by nine additions. Now, in the soss notation, the computer must observe that the distinct multiples which can occur are 13, but just as easily formed in that way, namely, the products by the eight digits from 2 to 9, and by 1o, 2o, 3o, 4o, 5o. But when the system is practised, all computers by it, if not learning (as they easily may) the table of multiplication up to 9 times 59,—quite as easily as children now do up to 20 times 20, will at least know the products of the nine digits into the 13 above multipliers; a table no larger than the present child's one to 144, but substituting different elements for those above 100, and expressing all above 59 sexagintally, thus :—

1	2	3	4	5	6	7	8	9	1o	2o	3o	4o	5o
2	4	6	8	1o	12	14	16	18	2o	4o	10o	12o	14o
3	6	9	12	15	18	21	24	27	3o	10o	13o	20o	23o
4	8	12	16	2o	24	28	32	36	4o	12o	20o	24o	32o
5	1o	15	2o	25	3o	35	4o	45	5o	14o	23o	32o	41o
6	12	18	24	3o	36	42	48	54	1Oo	20o	30o	40o	50o
7	14	21	28	35	42	49	56	103	11o	22o	33o	44o	55o
8	16	24	32	4o	48	56	104	112	12o	24o	40o	52o	64o
9	18	27	36	45	54	103	112	121	13o	30o	43o	60o	73o
1o	...	...	...	...	...	...	...	...	14o	32o	50o	64o	82o
2o	...	...	...	...	...	...	...	...	...	64o	1oOo	132o	164o
3o	...	...	...	...	...	...	...	...	...	...	150o	200o	250o
4o	...	...	...	...	...	...	...	...	...	...	...	264o	332o
5o	...	...	...	...	...	...	...	...	...	...	...	...	414o

To the left of the upright line are only the products learnt by all children, but with those exceeding 59 put into new notation. Right of the line are to be read that *twice* 20 *are* 40; *twice* 40 *are* 1 *soss* 20; *seven times* 30 *are* 3 *soss* 30; *twenty times* 40 *are* 13 *soss* 20; &c.

The only operations in arithmetic that present any difficulty at first, compared with their working in decimals, are the extraction of roots. The cube-root evolution, even in the denary scale, though always described and taught, may be hardly regarded as a practical rule. Logarithmic or other tables are the commoner resource; or some of the easier approximate methods for this and all higher roots taught in algebras. But the square root extraction is a practical problem, and in applying it to a sexagintal number we have to treat each couplet of figures as a single denary figure is treated, and therefore to point them off into pairs of couplets. Two figures at a time must be appended to the root, and four at a time brought down from the dividend. If the latter, pointed off, gives more than two figures before the first point, it is necessary to know or have a list of all the first 59 squares with their roots; like that which has been deciphered as the content of one of the Assyrian tablets, in cuneiform numerals; and which proves the Ninevites to have been quite up to working this problem.

With regard to the familiarizing of this arithmetic among us—not only does it involve no new or strange character, nor even unfamiliar symbol—but it seems singularly lucky, that nearly all printers have now numeral types admirably fitted to distinguish the alternate larger and smaller figures. For the smaller we should use what they term the antique form (as in the *Echo* newspaper), the tops of the 3 5 7 9, and both tops and bottoms of the o 1 and 2 ranging with the ordinary minuscule letterpress; while, for the alternate large ones, we have the 1 2 3 4 5 0, all of the same height with capitals; which, during the first half of this century, had superseded the more legible numerals of unequal heights that are now being revived. But, as the 6 7 8 9 of tall digits would not be used, their 6 and 8 would be in no danger of confusion with the G and 8, the only figures of similar height in the small decade. Of course the alternation of these two sorts of figures would at once mark off sexagintal from denary numbers, and leave no possibility of mistaking, in any line, which system was employed. The two are as distinct to any eye as the capital and small alphabets.

The main object of reviving this most ancient known, but (as I trust it can be shewn) most civilized numeration, which indeed has never quite died out from scientific use, and has left ineffaceable traces in every known nation, would be to have a simple or continuous arithmetic to which possibly all our monetary and metral tables might be assimilated; as the Chinese money and measures are said to be assimilated to decimalism, and as the French have

vainly attempted to force theirs into the mould of the same crude and random result of cultureless finger-counting. I say "vainly," because they have long abandoned the ten-day wage periods that were to replace weeks, and were a fundamental feature of the grand harmony ushered into the world in the name of the "Goddess of Reason:" they have left untouched the hours and minutes of the day, so that no *time*, great or little—a most fundamental article of commerce—can be bought or sold decimally at all: and even the production of vast new trigonometric tables, and the example of so real a man of science as Laplace, has utterly failed to displace the ancient Chaldean division of the circle and sphere by a decimal one. Sir J. Herschel, in a note to his essay on the *Yard, Pendulum, and Metre*, states that the decimal division of the quadrant introduced in France, was abandoned by common consent even in France, and can never be reintroduced. The attempt has been made in the fullest possible manner, with no parsimony, and with every conceivable start, that the political power of a Napoleon the Great and the science of a Laplace could give it; and three generations of such exhaustive trial have shewn, if any experience ever can, either that the sexagintals, wherever once established, are too good to be displaced, or that finger-counting is too retrograde and anti-scientific to displace them. Of the latter fact, I cannot entertain the smallest doubt; but the former is not yet clear. The continuous sixties may be too superior for every purpose to the continuous tens, ever to be displaced thereby, or even by any other *continuous* repetition or alternation of identical multipliers; and yet be found in commerce displaceable by the advantages of some *discontinuous* table of divisions, formed, as all our old money, weight and measure tables have been with more or less art, purposely to avoid repetitions of identical steps, and by introducing not merely the first three primes 2, 3, and 5, but occasionally higher ones, to render the larger units more divisible, or into a greater variety of fractions than even considerably larger units in any continuous or repetitive numeration would have.

It may here be said by a decimalist, that not the continuous sexagintals, but even the decimals, that I despise as so greatly their inferiors, *have* displaced in France, for ordinary subjects except time, the compound tables that I am praising;—say rather, whose principle I am praising, for there is no existing table that I regard as carrying out the principle artificially enough. This, however, does not represent the historic fact in France, which is that decimalism displaced not a few distinct and conflicting tables like our

present imperial ones, but a host of local and utterly *disconnected standards*. And the same is the case in Germany and all other countries that are now following her.

A reference to the tables printed under the word *Measure*, or *Weight*, or *Pound*, in Hutton's *Mathematical Dictionary*, or many Cyclopædias, will shew that our present variety of units, each common to all Britain, is nowise parallel to the chaos of local city standards yet in use through most of Germany and Italy. The fact is, that the total neglect of all theory on these matters, by all rulers between the eclipse of Roman civilization and 1790, allowed the barbarizing and multiplication of random standards and trade customs to reach a pitch, that left the French Resolutionists no time for inquiry, or aught but this most retrograde and headlong plunge into the expedient nearest at hand. Accepted as a *pis aller*, it should have been matter of national humiliation, instead of being trumpeted as a triumph of Reason, and model for general imitation. The derivation of all other standards from that of length was nothing new. All civilized peoples of antiquity had this, as a matter of course. The only novelties were the *intended* relation of the length-standard to a dimension of the earth (which relation, admittedly, *has not been effected*), and the unprecedented clumsiness of weights, measures, and money, whereof no unit may divide by 3, or anywise but by powers of 5 and (with great complication) those of 2. Even the boast that a Frenchman might be able to say his field is such a fraction of the earth, has not been realized, and would not have been, had the *mètre* been theoretically perfect; for neither square nor circular mètres are commensurable with the globe's surface. This would have required (even on a globular earth) a standard derived from the radius instead of the quadrant—but the ellipsoid form precludes it altogether. The standard the modern world is actually to take, is not even a noble and durable *artificial* one; not even embodied, like the Greek foot or Egyptian cubit, in highly-finished architectural monuments, as measurable now as when wrought twenty or thirty centuries ago; and as our English long measures have been for six centuries, in fanes of less exact workmanship, but still noble. It is simply the length of a short metal bar kept at Paris, just as the "Winchester bushel" means the capacity of a certain bronze vessel at Winchester. Both are equally destructible, and of course the bushel holds an ascertainable fraction of the earth's solidity, as the Paris bar does an ascertainable fraction of her quadrant. Neither of these fractions will be round, in decimals or any other numerical scale.

The whole pre-eminence, then, for which mankind are to follow the Paris bar, consists in this, that the said bar was, by certain philosophers, intended to be (but *is not*) a decimally round portion of the quadrant!—a relation useless, had it existed.

The necessary and perpetual advantage of compound arithmetic over simple; that is, of compound tables like our historic ones, of weights, measures, or money, over any simple, or as Mr. Napier has called it, *continuous* scale, that is, any that repeats the same multiplier, becomes obvious from the very name acquired by one of the rules in our school arithmetics. One rule is called *Practice*, implying it to be, as it truly is, the sole method really employed in all the practical calculations of commerce. The methods of obtaining the same results by long multiplications and divisions, or both combined, have first indeed to be taught, but merely to ground the learner in the principles of the subject, after which this abbreviated method so naturally presents itself as hardly to require to be taught. Now, all the value of this notoriously practical method plainly hinges on the variety of divisors or aliquot parts that can be found in the chief unit of each table, as the pound sterling, the ton, &c. Lists of these aliquot parts are learnt by rote, under the name of *practice-tables;* and it is worth while to compare the extent of some of these with the lists that would be the largest yielded by powers of 10, to which the Chinese and French system, miscalled *metric*, would confine us; or by the 144, 1728, &c., that the duodecimal produces. In all treatises on the "Theory of Numbers," as that of Legendre or of Peter Barlow, one of the earliest demonstrations is that of a theorem for finding how many divisors a given number can have. If a prime, of course it has but two, itself and unity. If compound, it is composed of, or is the multiple of a certain number of primes, each occurring singly or repeatedly. Thus, if the pence in a pound, or 240, be the number proposed, we find it divides by 2 four times, and the final quotient, 15, is the product of 3 and 5, the next two primes to 2. It is, therefore, when fully decomposed, represented as $2^4.3.5$. The indices of these three factors, namely, 4, 1 and 1, must each be augmented by unity, giving 5, 2 and 2, and then the product of these, or $5 \times 2 \times 2 = 20$, is the number of divisors that the given number 240 admits, including always itself and unity as two of them. It will be found that, without splitting pence, a pound has 18 aliquot parts between itself and a penny, or 19 including the penny. Now, decompose a French "metric" thousand of anything in the same manner, and we find 1000 (like every power of ten) has but the two factors 2 and 5, in

this case cubed. It is to be represented as $2^3.5^3$, and the indices of these, augmented by 1, are $4 \times 4 = 16$. Accordingly, a ten-franc piece, though divided into 1000 centimes, more than *four times* as many as our pound contains of pence, yet yields fewer aliquot parts by four. Its "practice-table," down to a centime, will have but 15 lines, where ours down to the penny (and without breaking pence) has 19. And this is but a very small example of the advantage, which becomes prodigious in comparing larger divided units.

All the numbers expressing, in old historical measure-tables, the ratio of one unit to another, are remarkably divisible; though not always, like 12, 24, 60, or 360, the very *most* so that could be devised within their range. Take, for instance, the inches in our mile, which are 63,360. This number, we find, divides by 2 seven times, then by three twice, and by 5 and 11 once each. Its complete decomposition yields $2^7.3^2.5.11$, and the indices of these factors, each augmented by 1, give 8.3.2.2, whose product, 96, is therefore the variety of aliquot parts into which the mile is divided, or rather 95, omitting itself. But this result we see depends only on the indices of the four factors, and is independent of their own magnitudes, provided only they be four different ones. We might, therefore, remove the factor 11, and substitute 7, the next larger to the remaining ones, without affecting the above result. In other words, our mile reduced as 11 to 7, or to 1120 yards, would still have 95 aliquot parts, as it now has. But a further reduction may be found that would still leave it as divisible. The indices of the primes 2 and 3, are respectively 7 and 2, which augmented each by 1, give $8 \times 3 = 24$. Now, the former index may be lowered to 5 and the latter raised to 3, giving when augmented, $6 \times 4 = 24$, and the number has received only *one* more repetition of the factor 3, while losing two repetitions of 2, it will be reduced in the ratio of 4 to 3, or from the above 1120 yards down to 840, still retaining as many divisors as the mile of 1760. Nor is this all—suppose it divided by 12, that is, by $2^2.3$; and the above two indices are reduced from 5 and 3, to 3 and 2. These augmented and multiplied together, give $4 \times 3 = 12$ the full half of their former product. But restoring the factor 11 has the effect of introducing a fresh doubling, and will, therefore, compensate for the loss of 12; so that eleven-twelfths of our last result, or an unit of 770 yards, or $3\frac{1}{2}$ furlongs, would contain a number of inches as variously divisible as the number in a mile. But below this, we shall not find further reduction possible without loss of metricality.

It appears, then, that a particular series of numbers from 2 upwards may be found, possessing each of them more divisors than any below itself possesses; and which series, from their excellence for measuring, weighing, and valuing purposes, we may call "*meristic*" numbers, that is, pre-eminently divisible, or having the utmost variety of parts. And these are evidently what the contrivers of old metral tables were always seeking, and produced with more or less completeness. I find that between 1 and 1000 are 14 meristic numbers; between 1000 and a million, are 23; between a million and a milliard, 28; and between a milliard and a billion, if I am not mistaken, are but 28; though the process of continuing their series becomes increasingly exacting of caution, and beyond nine figures it is easy to overlook some. They necessarily advance by ratios on the whole decreasing, so that, between the successive powers of any considerable root as 1000, there will be more and more of these steps. I will here give the meristic numbers between 4 and a million, with the ratio that each bears to the next above it.

No.	Ratio.	No.	Ratio.	No.	Ratio.	No.	Ratio.	No.	Ratio
6	1:2	180	3:4	2520	1:2	27720	11:18	221760	4:5
12	1:2	240	2:3	5040	2:3	45360	9:10	277200	5:6
24	2:3	360	1:2	7560	3:4	50400	10:11	332640	2:3
36	3:4	720	6:7	10080	2:3	55440	2:3	498960	9:10
48	4:5	840	2:3	15120	3:4	83160	3:4	554400	5:6
60	1:2	1260	3:4	20160	4:5	110880	2:3	665280	12:13
120	2:3	1680	2:3	25200	10:11	166320	3:4	720720	2:3

This short list, easily verifiable, will suffice to shew that the prevailing numbers in ancient tables, as those of pence in a pound, a crown, or a shilling, hours in the day and night, minutes, degrees in the circle, or degrees of heat between Fahrenheit's thermometric points, were not blundered into at random, or as our friend Mr. Napier has surmised in the case of 180, that it had "no peculiar properties attached to it—none that we were aware of—more than 100 or 250." On the contrary, all these metral or meristic traditions, dating, as Sir Henry Rawlinson tells us the sexagintal numeration did, from immemorial antiquity, are the fruits of profound thought and real science, among ancestors who had rather more thorough views of what they were about, than to hold that a number's being the square or cube of the sum of their fingers was a reason for subordinating all measurement and all division and valuation to it. The fingers, indeed, had so far affected early numeration, that probably the sexagintal system would not have arisen had the fingers

not been five. We find a trace of its mode of origin in the Chinese cycle of sixty days, and also that of sixty years, which, for them and all Eastern Asia, takes the place of our century. The names distinguishing each of the sixty are composed of two syllables or words, one of which recurs at every twelfth day or year, and the other at every tenth; shewing that two independent cycles have been used—one of the meristic number twelve days or years, and the other of the number of the fingers. The same pair, out of these two sets of names, could only come together every sixtieth day or year, and thus the separate periods, denary and duodenary, merge into a single sexagintal one. If we had been sex-digital (as some hasty writers on this matter tell us would have been desirable) all our notation would have been either senary or duodecimal, and never any better. It would have slightly excelled the denary scale, in about the ratio, as we have seen, of 699 to 477; but the sexagintal system, which we have proved to transcend infinitely the utility of both, would never have been developed; as the mere sight of fives and tens in the petals of flowers or starfish would hardly have suggested the taking an additional prime factor into the numeral scale. Instead of saying, then, with Sir William Thomson, "as it is impossible to introduce the improvement of twelve fingers, I am afraid we must continue bound to the decimal system," I both deny that our fingers could have been better, even for arithmetical suggestiveness, and fail to see that they can bind us, in civilized times, to decimalism, to which they never bound the ancient Chaldeans or Hindoos, or any of our European teachers in liberal arts, from Hipparchus, Vitruvius, and Ptolemy, down to the eve of the French Revolution. Is it the outcome of three thousand years' boasted progress to abolish, for the first time, all art or reason in these matters, as too weak against the tremendous fact of our number of fingers, and to enslave all transactions at length to finger-counting like that of Africans? Surely the favourite title that the originators of this decimalist rage chose for themselves, that of "*Sans-culottes*," is the most correct name for the wretched system, instead of vaunting it as anything "scientific."

It may by some perhaps be objected to the antiquity claimed by Sir Henry Rawlinson for the soss notation, that the constant use of the denary method throughout the Bible proves it to be the more ancient. It proves, however, no such thing. Sir Isaac Newton, whose now neglected posthumous writings on bibical criticism, went, I would beg to observe, very much further than anything so called among us at present; and however condensed, contain (according

to Bishop Horsley his editor, who had seen all his notes) the fruit of the greater part of his life's work,—Sir Isaac places it beyond all doubt that no part of the present Bible, however ancient may be its *matter*, is any older in *form* or *language* than the reign of Saul. Its method, therefore, in any such matter as arithmetic, merely proves that such was the method current in Canaan at that epoch; barely a century or two before Homer. The compilers of the present Pentateuch, either Samuel (as Sir Isaac Newton shewed to be almost certain) or pupils left by him in his college of Ramah, while collecting and putting the Mosaic and more ancient remains into the Canaanite language (now called Hebrew), necessarily turned also their numbers into the notation best understood by contemporaries, if they were not in such already. And it was hardly possible they were not so already; because the nation had, for all the centuries since Abraham (or since they were a single household) been in Hamite countries, either Canaan or Egypt; and as inevitably subject to the languages, arts, and whole civilization thereof, as Jews now are to those of whatever lands they inhabit. Their letters and arithmetic they had exclusively from the very same two tribes whence the civilizers of Europe had theirs, namely from two branches of Ham's posterity, the Mizraite and Canaanite or Phœnician; so that bibical numeration is necessarily whatever ours is. Nothing but confusion can involve such matters till we bear in mind what Dr. Beke has striven to impress on us, that since Abraham's time, his descendants have been Shemite in pedigree alone, never in arts or language; it being an entire error to call the biblical tongue (which the Bible itself calls "language of Canaan") or any of its congeners, "Semitic." The traces of whatever is really Shemite, in language or arts, must be looked for in more eastern tribes, from Assyria to China. Now it is in them that the sexagintal arithmetic has left the most evident remains.

To return to the decimalist rage, a writer in the *Saturday Review* lately was so far from seeing anything in our fingers to "bind us," that he goes to the opposite extreme of questioning whether it would not be as easy to duodecimalize our whole arithmetic, as to bring all weights, measures, and money, like our Continental neighbours, into the inferior and barbarously inconvenient decimals! Of course it would be much harder, involving the use of new characters for 10 and 11, and entire revolution of all ciphering above the unit 9. The innovation would be vastly greater and more difficult than sexagintalism, and yet bring infinitely less advantage. The "gross," or 144, is not among the meristic numbers, nor is

even its half, nor more than very few multiples of it; so that though all the meristic numbers above 12 are multiples of 12, and would thus, in the *Saturday Review* notation, end with a cipher, hardly any would end with more than one. But not only must all the meristics above 60 be multiples of 60, so as to end, in the soss notation, with 0o, but many also with four ciphers; as the numbers 25200 and 50400, which are so round sexagintally as to be expressed by 70o0o and 140o0o. It is true that no perfectly round units in any notation can ever be meristic, because they are powers of the base; and it may be proved that none of the meristic numbers can be an exact power, beyond two singular exceptions, in the beginning of their series, the squares of 2 and of 6.

The only number above 60, sharing with it this property, that all meristics above it are multiples of it, will be found to be 2520, or 42 sixties; this being the last in the above table that bears as large a ratio as 1 : 2 to the next meristic above it. We may rigorously prove that no number above 2520, can require (as this and 360, and 60, and 12 do), a full doubling before gaining more divisors; so that this is the highest which can divide in as many ways, as all numbers below its double will. Now this notable meristic quality attaching to 12, 60, 360 and 2520, but no higher number, makes it questionable whether, even were our simple arithmetic sexagintal, it would not be desirable to break the continuity of this (the very best continuous) scale, by having in the standard tables of weights, measures and money, an unit equal to 42 soss, or 2520 (in denary notation, but then written 42o0) of the smallest units. This would introduce, in the most advantageous way, the fourth prime divisor, 7, which the contrivers of various ancient tables thought worth introducing. This factor is a multiplier in our *avoirdupois* weight and *wool* weight tables, and in the Russian long measure (founded on our own standards) while our own long measure, equally quater-primal, curiously skips over the 7, to take the next prime, the 11. Our money-table, however, was formerly so meristic as to embody the first four primes, and this so perfectly as to make the highest unit precisely the above remarkable number, 2520, of the lowest. This was a result, I believe, of mere accident, the guinea and five-guinea piece having been intended for only 20 and 100 shillings, but through a miscalculation, having been coined worth 21 and 105. Still the fact is truly remarkable, that since our practically lowest coin has become, as it has for centuries, the *half-penny*, the very highest English coin yet struck passed for 2520 half-pence; the precise

number that pure mathematics can prove to have the above meristic pre-eminence.

If our coin, already much nearer to a sexagintal than to the Sans-culottes' system, were to be modified in the easier and improving direction, instead of the harder and deteriorating one; and instead of purely decimal, to be made purely sexagintal (like the Hindoo divisions of the day, and our own of the hour and degree); then, the half-penny being evidently our established minim, only requires a simple name, as *obol;* and sixty of them we already have as an unitary and common coin, though with only a clumsier name, *half-crown*, than perhaps any other civilized nation would perpetuate. But the next sexagintal step, a gold piece of £7, 10s., would be too heavy for a single coin. Now, if the remarkable meristic unit formed by 42 soss were introduced, instead of squaring the 60, this would be merely reviving, as the new chief monetary unit, the largest and finest already in our history, and most worthy of a rich nation, the five-guinea piece. Only it would, of course, need a simple name, indeed a monosyllabic one, if it is not to be superseded by nicknames. For our people will never be brought to name even by dissyllables anything common and important. Two syllables they will allow to female names, or to things, like a shilling, not so grave as to transcend play. There are classes by whom even that coin is felt to require a masculine monosyllable, "bob;" and to none of us are *pounds* facile or undignified enough to be dissyllabled. For any higher coin, then, I would suggest *min*, from the classic and scriptural *mina*, the root word of mint and money.

The great first question that remains open in these matters I take to be evidently this: whether the best continuous or simple arithmetic, which is demonstrably the sexagintal, would be meristic enough to enable us to dispense with all compound scales. Ancients thought *not* so, or we should have inherited no *avoirdupois* weight, and other tables involving 7 as a factor, and even 11. The Sans-culotte followers, on the contrary, would make even a far inferior continuous scale, the random accident-sprung decimalism, supersede all compound ones. This I think out of the question. No tyranny could long force men to forego in all commerce divisions by 3, the lowest odd number. No simple arithmetic inferior to the very best (if even that), can supersede all compound ones. The duodenary could not, and far less the denary. Whether the sexagintal could, I submit as the first open question.

In any case, however, we should, if needing compound tables, plainly have only one. Whatever is the most meristic table

is simply the *best*, for one thing as well as another, money as well as weight, long-measure or time. All should have a table of the same steps, only differing in the names of the units. The fixing this best table would be the second question, supposing the former settled in the sense that a table more meristic than the number 60, *i.e.*, more than ter-primal, is desirable. But a singular question would then arise, which I have not yet touched. The three primes next above the first three, are 7, 11, and 13; the two former, we have seen, taken into certain ancient metral systems, but the last into none that I know of. Now the product of these three primes, $7 \times 11 \times 13$, is 1001; which number accordingly divides the last meristic number in the above table, 720720, the highest in the first million. It also follows that all higher meristics must be multiples of 1001. But this number has a singular connection with our denary scale, in which scale it is plainly one of the easiest possible multipliers. The most meristic number in a million, or ever likely to be adopted as a unit in any table, namely, 720720, whose "practice-table" would embrace 239 aliquot parts, would split, we see, into four such easy steps (decimally speaking) as 8, 9, 10 and 1001; and the idea suggested is, that if a general compound table for the measures, weights, and money, were to be thought worth carrying to this degree of meristic excellence, it might be worth while to prefer our denary notation (already possessing the ground), to the sexagintal itself, merely for the ease of working with this divisor and multiplier, 1001.

But this is the sole case I can conceive that would excuse or justify an adherence to our traditional decimalism; and after all, we could never hope to either decimalize or bring to this novel 720720 the divisions of time, the circle or angles.

I have not touched the question of metral *standards*, which is purely physical, and which, Sir John Herschel well insisted, should be kept entirely unconfused with the mathematical one of division. This, which may be called the *Meristic* question, is certainly the prior one, and I hope to have drawn attention to a few of the points neglected in connection with it.

PROPOSED SEXAGINTAL UNITS.

1,0o0o0o,0o0o0o, = One Sarrion.	=	46,656,000,000
1o0o0o,0o0o0o, = Ten Soss Nerion.	=	7,776,000,000
10o0o,0o0o0o, = One Soss Nerions.	=	777,600,000
1o0o,0o0o0o, = Ten Nērions.	=	129,600,000
10o,0o0o0o, = One Nērion.	=	12,960,000
1o,0o0o0o, = Ten Sares.	=	2,160,000
1,0o0o0o, = One Sare.	=	216,000
1o0o0o, = Ten Nēres.	=	36,000
10o0o, = One Nēre.	=	3,600
1o0o, = Ten Soss,	=	600
10o, = One Soss,	=	60

o·1 = One Sixth, or Ten Lepts.
·01 = One Lept.
·0o1 = Ten Nērets.
·0o0 1 = One Nēret.
·0o0o1 = Ten Sarets.
·0o0o0 1 = One Saret.

·0o0o0o,1 = Ten Nerionets.
·0o0o0o,01 = One Nerionet.
·0o0o0o,0o1 = Ten Soss-nerionets.
·0o0o0o,0o0 1 = One Soss-nerionet.
·0o0o0o,0o0o1 = Ten Sarrionets.
·0o0o0o,0o0o0 1 = One Sarrionet.

SIR WILLIAM THOMSON, who was prevented from being present, sent the following note:—"Mr. Garbett's estimates of values of different scales are very ingenious and carefully worked out, with much appreciation of certain parts of the scientific questions involved; but his suggestions are quite unpractical. Nothing but mischief could, in my opinion, result from any theory of a notation different from the ordinary Arabic one, coming between the (unenlightened) public, and their acquisition of the French metrical system. This acquisition *can* be and *may* be perniciously retarded; IT CANNOT BE STOPPED.

Mr. JAMES R. NAPIER stated that although the subject was not quite so new to him as to most of the members, it was sufficiently novel to prevent him from expressing any strong opinion of the matter. It must be evident to all, however, that what Mr. Garbett

had brought forward and discussed is one of the matters which ought to be seriously considered by those best able to judge of it before committing us to any particular system of weights and measures. The division of the circle was and is evidently the stumbling-block in the metrical system, and Mr. Garbett, with others, considers that this is the first point to be settled, if it is not already settled by the division which has continued in use in some things for thousands of years. So far from agreeing with Sir William Thomson as to the unpractical nature of Mr. Garbett's method of working the scale of 60, he (Mr. Napier) thought the system particularly practical and simple. He believed that if he studied the subject for a night he could get so familiar with the system as to work easily any arithmetical question. The only doubt he had was whether the importance of the method called "Practice" in many arithmetics was such as to render the change desirable. He had personally used that method but seldom in ship-building or engineering, and he was not aware what rule or method retail merchants or the mass of the population followed, so as to make his opinion on that point of any particular value. Nevertheless he contantly preferred a simple vulgar fraction, which the practice method virtually is, to a longer decimal one. He was not aware until he had seen Mr. Garbett's paper that any other system than decimals was possible, except by the introduction of new characters. He had advocated a continuous scale, such as the denary, without any other proof than his own experience had taught him, nevertheless the scale of 60, worked as Mr. Garbett proposed in steps of 6 and 10, and extended to *measures of everything*, he thought had every appearance of being a decided improvement on every known system, and to his mind more like what a universal system should be than anything yet proposed. Mr. Napier admitted his ignorance of the origin or use of the apparently discontinuous scales in our weights and measures, but submitted that Mr. Garbett's theory of the matter, so far as it related to Fahrenheit's 180° between the melting of ice and the boiling of water, was not so probable as the one which makes that number the expansion of Mercury between these temperatures, when the volume at melting ice is 10,000.

Mr. E. M. DIXON, in reference to Mr. Garbett's paper, said that for practical calculation the using of 10 as a divisor partially combined with the use of 6, was hardly so convenient practically as our present scale. He had been always led to believe that the great advantage of the decimal system was doing away with the rule of

practice, which was by no means so valuable a rule as had been indicated. He would say in one word, let us have scales such as Mr. Napier has proposed, then the decimal scale answers the purpose. If you introduce any other scale you have a host of new names and new letters to learn of which there is no use.

Mr. SMITH said,—The decimal scale is a little more popular than most people think, and it has often struck me that the reasons why it was not adopted in this country were two,—first, the fixing of the unit, and second, the introduction of the element of making it a national question.

Mr. Napier read the following Extracts of Letters he had received on the subject of Weights and Measures since it was last before the Society:—

From Mr. Lewis Gordon, formerly Professor of Engineering in Glasgow University,—"I have read your paper and been much interested, instructed, and amused. The sad result, however, is that I accept Sir William Thomson's reasonings and conclusions! If we ever change our system of weights and measures I am all for the mètre. But I feel certain that in *our time* the system will not be changed further than it is at present. The two-foot rule is too deep ingrained in our habits."

From Mr. E. L. Garbett,—"That the quality of having all our standards of value *connected*, or singly derived from that of length, was in fact so obvious and universal in ancient systems that we cannot suppose it to have wanted naming, till now that the dark ages had thrown all into confusion, and led to countless *disconnected* standards, a Winchester bushel, a pound of this market, gallon of another, &c., each an original random quantity. Every ancient length standard necessarily led, on one hand, to land measures of area, on the other to dry or liquid capacity measures (usually, but not necessarily, cubes, they might be cylindric or spheric),—and these again (filled with *water*) were units of weight,—and the same units of weight, in gold or silver, units of value. No people would surely ever think of making a standard system *without* this general connectedness. But that involves, in our case, the abandonment of at least *either* the 'avoirdupois' *or* the long measure standards. *Both* cannot be retained, with one-basedness, let the numeral system be what it may. But the long measure is by far more mixed up and pervading our language and laws and customs than any *weight* standards can be. To alter it would be incomparably more innovat-

ing and radically confusing than a new weight. Moreover, it is actually, as Herschel shewed, the most widely used now on earth; more so than the mètre, even when all Europe but we, Russia, and Scandinavia shall have the mètre! Our foot is the length standard for all lands north of our own parallel, all North America, the Russian Empire, and India! The mètre will never prevail over a quarter the space that ours does already."

From Staff-Captain Evans, of the Hydrographic Department of the Admiralty, in answer to an enquiry as to the divisions of ships' compasses (the divisions of our own being as is well known like the $\frac{1}{4}$-gill, binary to the seventh degree),—"During my official career I recollect several proposals for a change of the present compass division, based on a decimal division. Whatever could be assigned as the merits of such a change would, in my opinion, not compensate for the general confusion that would be introduced. All the western nations adopt the 32 points, and, so far as I know, also all the eastern, including the Chinese. If the latter people ever used a different division they have certainly now equivalents to *our own* points of the compass. The Japanese have *equivalents* for our 32 points, but in their charts they divide the quadrants into six parts, or 15° to the point. I am uncertain whether this practice is general among them, or whether it is ancient or modern."

From Dr. T. R. Robinson of Armagh,—"As to the measure of length, one which, like yours, preserves two of the old standards of the country, sacrificing only one, must be preferred to any which upsets everything. I do not think that the smallness of your grade as compared to the yard is any valid objection. The difficulties which beset its use lie, I think, in the nomenclature. That of the metric system, besides the difficulty to workmen and others mastering such a jarrugo of new words, has the fault of being barbarous in its Greek portion. Hecto, which they use to represent a hundred, means the sixth, and kilo has no existence in Greek, it should be chilio. I think it would be desirable as far as possible to denote the decimal multiples by single words, as pint, gallon, &c., 10 gallons = keg, 10 kegs = puncheon (or something else). For the descending scale, a similar plan might be adopted in preference to names of such a technical character as those of the French system."

The PRESIDENT remarked that there were three things to be considered in any discussion of this subject. First, there was the physical standard of measure to which an appeal might be made in the event of the existing measure being destroyed or defaced; then

there was the establishing of a simple numerical relation between the standards of length, capacity, and weight; and finally there was the sub-division of the three standards according to the decimal scale. Mr. Napier did not seem to attach much importance to the first of these questions. As regards the second, he proposed to retain the pound and gallon, and to combine with them a standard of length, which he calls the gradus. In this way he would bring the three standards into complete harmony as far as simplicity in arithmetical operations is concerned. As regards the third question, Mr. Napier appears to appreciate fully the advantages of the decimal scale, and would approve its introduction into this country. The President considered it to be exceedingly desirable that the unit of measure should be based upon some physical standard, and he regarded Sir John Herschel's idea of employing the polar axis of the earth for this purpose to be worthy of adoption. At the same time he considered it to be a very trivial advantage of the scheme, that the earth's polar axis contained almost exactly half a million of inches. It appeared to him to be quite immaterial how many inches were contained in the earth's polar axis in so far as regards its applicability as a standard of measure, so long as the number of such inches remained constant. In astronomical researches the unit of lineal measure was either the earth's radius or the radius of the earth's orbit, but it was only very rarely that there was occasion for expressing either of such units in terms of miles, feet, or inches.

Mr. Napier's paper contained many excellent ideas, and by his proposed new measure the grade, he had brought into harmony the three standards of length, of capacity, and weight. This result however, was effected only by the sacrifice of the existing measure of length, which must be regarded as the fundamental standard of the three, and it did not appear probable that the public would consent to such a sacrifice.

XVII.—*On the Purification of Water. By* G. BISCHOF, *Professor of Technical Chemistry at the Andersonian University.*

Read before the Society, January 22, 1873.

THE observations and results of experiments which I beg leave to lay before you refer merely to water, which is to be employed for domestic use, and not to the purification or utilization of sewage. Perhaps I ought to apologize for choosing this subject, which is not of immediate interest for those residing in Glasgow, as we are fortunate enough to have a water supply which requires no purification. However, this question is of grave importance to by far the majority of fellow-creatures, who have no Loch Katrine, and whose water supply is derived from at least doubtful sources. We know that in 1852 an Act was passed for the more effectual filtration of water supplied to towns and cities. But this Act has not prevented one of the London Companies (as admitted by their Engineer, in an official enquiry in 1869) from distributing to the general district about one million gallons of tidal water, taken from the Thames at Battersea. It could not prevent, as we are informed by Dr. Frankland in his Report of April 24, 1871, the collection of a mud to a depth of at least two feet at the bottom of a reservoir belonging to one of the Companies. The coarser portions of this mud, after being thoroughly dried, were found to contain 13·72 per cent. of nitrogenous organic matter in a putrescent condition. A few months ago a case of typhoid fever with fatal result was traced from London to Leamington. An analysis of water of the latter town, made by Dr. Letheby on behalf of the local authorities, disclosed an excess of organic matter, which he reported might be possibly due to temporary floods.

These instances, which might have been multiplied by many more, will, I trust, be considered a sufficient excuse for bringing this subject before you. The impurities in water to which the attention of the analytical and practical chemist should especially be directed are suspended matter, dissolved fermentable organic matter, and certain mineral matter.

The purification of water can either be completed by Water Com-

panies, or it may be wholly or partially effected by consumers in small domestic filters.

At present the only filtering medium extensively used by Water Companies is sand. Even by the most careful filtration of water through sand only suspended impurities can essentially be removed. The turbidity of water affords a test sufficient to decide the efficiency of such a mechanical filtration. The dissolved impurities are at present, in practical working, beyond the control of any Company, unless they can modify their water supply. I say, in practical working, because even by filtration through sand a chemical purification from dissolved impurities may be attained; but this would, according to Dr. Frankland's researches, require such a thickness of the filtering medium, or such an extremely slow filtration, that it is practically impossible.

While conversing with the Engineer of one of the London Water Works during a recent visit, it struck me that the sand, which, after being washed, is used over again for filtering, might be one of the causes of the apparent deterioration of water. The arrangement of these sand filters is well known. At the bottom of the tank constructed for the filter are placed layers of pebble stones, gradually decreasing in size until they come to the size of the coarse sharp sand which is placed on the top. When water is passed through such a filter, nearly all the suspended matter in it is deposited on the very top of the sand, where it gradually chokes the filter. The top sand and dirt is then removed by scraping off about half an inch of the surface. This surface sand or mud is washed with water. It falls from a tank, in which an upward current is produced by water entering at the bottom under pressure, through an inclined sieve, which retains the coarser impurities. From the sieve the sand falls on to an inclined trough, over which the water, which passes through the sieve, runs. The coarser particles of the sand are constantly worked back on the incline of the trough with a spade, while the finer particles and the mud are carried away with the water and run to waste. The sand thus recovered is used over again. I could not gather whether this washed sand had ever been analyzed for its purity; so I took a sample, which I was informed had been used over again some twenty times, and another sample of the same quality of sand, which, I was told, had never been used for filtration before. The result of my examination is satisfactory, shewing that from this source no danger need be apprehended.

Both samples dried at 100° C. were heated to redness. The sample which had not been used, lost on heating 1·0 per cent.; and

the sand which had been used even less—0·9 per cent. The former, on heating, exhibited the smell of sulphurous acid, but did not turn black; the latter, which had been used and washed, betrayed no smell of sulphurous acid but turned black. Fifty grams of each sample were then mixed with ½ litre of Loch Katrine water, and frequently shaken during 24 hours. 100·000 parts of the extract of the sand which had not been used, filtered through paper, gave 0·41 free and 0·48 albuminoid ammonia; the other even less—0·31 free and 0·36 albuminoid. Nevertheless, considering the facts above alluded to, it may be questioned whether it is satisfactory to rely solely on the filtration of water through sand—at least, if the source from which the water is derived is not above suspicion.

If we bear in mind that London alone requires some 1¼ million gallons in 24 hours, it is not easy to comprehend how a method could be discovered to deal efficiently and practically with such vast quantities. Moreover, considering the difficulties of any chemical purification on an extensive scale, it does not stand quite to reason, that the water used for flushing water-closets and the like should be purified equally by the Companies with the water used for drinking; and under any circumstances a chemical purification of the whole bulk of water by Water Companies would probably result in an *imperfect* purification. In the hands of consumers many plans may be practicable which would be utterly impracticable if applied to the large bulk the Water Company has to deal with; and the consumer can limit the purification to the comparatively small quantity required for culinary and drinking purposes.

Few plans only have been suggested, with any chance of success, for the chemical purification of water by water companies. Amongst these, Dr. Clark's softening process and Spencer's Magnetic Carbide process have been prominent. One of the advocates of the former process is Dr. Frankland, who explained, in the Minutes of Evidence taken before the Select Committee on Metropolis Water (No. 2 Bill, June 28, 1871), that Clark's process consists in mixing a certain quantity of milk of lime with the water as it is flowing into the reservoirs, and then allowing the mixture to subside for about 12 to 24 hours, passing it afterwards, in the case of river water, through the present sand filters. The effect of the process is, to reduce the hardness of the water to about ¼ of what it was previously, and to reduce the proportion of organic matter, according to Dr. Frankland, to less than ⅕, and the total solid impurity (or residue, in the ordinary way of speaking) to considerably less than ½. Dr. Frankland attributed the purifying action of Clark's process to a surface attraction. The very

fine precipitated chalk, he argued, attaches itself to the organic matter, —dyes itself, as it were, with it. The chalk that is thrown down from the Thames water is of a coffee colour, or nearly so, from the organic impurities which it carries down with it.

Taking for granted that the proportion of albuminoid ammonia resulting from a water, before and after softening, would give some indication of the ratio at which the organic matter is reduced through Clark's process, I recently softened a sample of the Southwark and Vauxhall Water Company's water, until turmeric paper gave just the faintest alkaline reaction. The conditions were then more favourable than they ought to be in practical working. The result was, that the water contained in 100·000 parts—

	Before Softening.	After Softening.
Albuminoid Ammonia, . . .	0·0307	0·0229

It will be seen from these figures that the albuminoid was reduced some 25 per cent.

Clark's plan has been tried, on a limited scale, by several Water Works; but the only localities, as far as I can gather, where it is, or at least was in 1871, in use are Tring and Caterham, two small places.

With this evidence before us it is difficult to see why this process, which appears to offer considerable advantages, has not met with more universal favour. If I may venture an opinion, a great drawback in working this process on an extensive scale is the difficulty of storing for an additional 24 hours the immense volume of water required, a difficulty which would prove almost insuperable in the case of those London Companies whose works are situated almost in the centre of the city. If the reservoirs for the treatment with, and the precipitation of the lime were to be made shallow, an immense area would be required; and if they were to be made deep, the deposition would take proportionately longer.

Again, if the deposition in these reservoirs be incomplete, the precipitation of the finely divided calcium carbonate on the top of the present filtering beds might seriously interfere with their working. I find some remarks bearing upon a similar question in an examination before the Judicial Committee of the Privy Council. Mr. Harper, the Secretary of the Southport Water Works Company, stated, that after working Dr. Clark's process for nine months, they found it was clogging up their pipes with a lime deposit, which rendered them useless, so that, in their case, it was a complete failure. It need not be remarked that for soft water, for instance—the Loch Katrine water—if it needed purification from organic matter, Clark's process would in no case be applicable.

The magnetic carbide process is the invention of Mr. Thomas Spencer. He prepares a porous magnetic oxide by mixing preferably the Cumberland hematite ore with a certain proportion of carbon, and heating the mixture at a dull red heat in retorts for 24 hours. The magnetic oxide produced is mixed with coarse sand when employed for filtering water, and it has on the top a layer of finer sand, some 3 inches thick, to intercept mechanical impurities, if such be present. Mr. Spencer states—and this is corroborated by the evidence of several witnesses—that a layer of from 6 to 12 inches of the magnetic material is generally sufficient to remove all organic impurities which are in a state of putrescence, while freshly dissolved organic matter takes longer. The products of decomposition of the organic matter are, according to Mr. Spencer, nitrites, or nitrates and carbonic acid.

It is claimed as *one* of the greatest merits of this material that its purifying properties are inexhaustible. This is substantiated, after a twelve years' trial, by the evidence of witnesses, such as the Secretary of the Southport Water Works Company and the Manager of the Wakefield Water Works; but I looked in vain for analytical data in support of the evidence. At the former place, the water obtained from a well sunk in the red sandstone, near Southport, contained in solution, besides organic impurities, a considerable quantity of iron, which, it appears, is also completely removed by filtration through the magnetic oxide. At Wakefield the supply of water is taken from the River Calder, which, about the year 1860, became very impure, owing to contamination by the sewage of a population of about 70,000, and the refuse matter of several manufacturing establishments. This water, as stated by the manager of the water works, has been rendered perfectly pure since the introduction of Mr. Spencer's plan, in 1864, and has ever since continued to be so. Here, again, I looked in vain for analytical data. I have myself made only a few experiments with magnetic carbide. I prepared it by heating hematite and charcoal in a large crucible until the first portions of the iron oxide were just beginning to be reduced to the metallic state. The purification of water by means of this product was not very marked; but some experiments with one of Mr. Spencer's filters, to which I shall refer hereafter, gave better results. This is probably owing to that gentleman's peculiar way of preparing his magnetic carbide.

Domestic filters for the purification of water by consumers should require very little attention—no frequent renewal of the filtering material, and the apparatus and the filtering material must be reasonably cheap. The number of domestic filters offered to the

public is much larger than that of the plans for filtering water by Water Companies. Omitting all those filters which can merely effect a mechanical purification by means of sponges and the like, the filtering material in most domestic filters is animal charcoal. I have had such a filter in experimental operation for three months, filtering barely a gallon a day of a water, which was certainly purer than any one of the London waters. The result was that, whilst at first the purification was extremely satisfactory, it very soon fell off, and the water, after three months, was actually rendered a great deal more impure by passing through the filter.

The experience I had with moulded charcoal filters was even more unsatisfactory, their purification being almost only mechanical from the very beginning. From the well-known fact that the purifying action of animal charcoal, which is the only purifying ingredient in these filters, depends to a very great extent upon the structure of that substance, this result might have been anticipated, considering the treatment the animal charcoal has to go through in the preparation of these filters. Professor Clark's process, as originally described by him (in 1841), could not be applied to domestic filters, as it required burettes, test solutions, test papers, &c. An appliance, described in 1864 by Mr. Danchell, for rendering the admixture of a constant proportion of lime-water to the water to be softened almost self-acting, might, in some simple manner, be adapted to domestic filters constructed on Dr. Clark's plan. It consists of an arrangement by which a certain proportion of the water supplied to the filter is made to pass through a compartment containing caustic lime in excess. The concentrated solution of caustic lime produced is reunited with the bulk of the water. The calcium carbonate in solution is thus precipitated and then retained by a mechanical filtering medium. The quantity of water which passes through the caustic lime compartment can be regulated.

In 1867, most of the filters offered to the public for sale by the various proprietors were subjected by the *Lancet* to an analytical examination, with regard to the purification of water effected by them. These filters were—The syphon filter of the London Water Purifying Company (Danchell's plan), in which the filtering material is animal charcoal; the moulded carbon filter of Messrs. Atkins and Son, the silicated carbon filter by Dahlke, and the magnetic carbide filter by Spencer.

The examination was conducted in the following manner:—A certain quantity of water, supplied by one of the London Water Companies, was passed through each filter, a litre of the filtered water

was then collected, and evaporated to dryness in a platinum dish. Towards the end of the operation some sodium carbonate was added in order to decompose any magnesium chloride present; the dry residue was heated to 120°-130° C., and weighed with the usual precautions. It was then incinerated, and again weighed with the usual precautions. Thus the following figures were arrived at:—

	Solid Residue dried at 120°-130° in 100,000 parts.	Loss on heating.
Unfiltered Water, ...	29·10	1·80
Danchell's Cistern Filtered Water, . .	14·90	·45
Moulded Carbon Filter, ...	31·27	1·10
Silicated Carbon Filter, ...	27·65	·90
Magnetic Carbide Filter, ...	28·45	·05

The figures are stated to agree with the observations made during the evaporation as to the smell of the samples and their behaviour on heating, with the exception of that filtered through the silicated carbon filter.

The only other test applied is that of potassium permanganate. Omitting the figures given, the result arrived at was that Danchell's cistern filtered water required (when quite new) the least quantity of oxygen for the oxidation of organic matter. After being in use for some time the effects were less marked. Next came the magnetic carbide, and then the silicated carbon filter, which latter required twice as much oxygen as the former. The moulded carbon filter gave, in one instance, slightly better figures than the unfiltered water; in another, almost the same figures.

The Select Committee on the Metropolis Water (No. 2) Bill, 1871 resolved, "that the Committee decline to enter into the comparative merits of different processes of purifying or filtering the water."

I am not aware of any other report of this kind. It is my intention to communicate, presently, some comparative results which I obtained in regard to the purification effected by a few filters; but, taking into consideration the large business done by the various filter companies, I beg to suggest that it would be extremely valuable if the merits of the filters, offered to the public, were thoroughly sifted by some impartial body vested with sufficient authority; and in my opinion this Society would confer a great boon on the general public, by entrusting a Committee with the task of examining the purifying action of such filters, and publishing their report.

I might continue these observations on filtering apparatus and media in general. But I wish to communicate to you to-night, some

experiments which I recently made, on the action of metallic iron on impure water, and I am afraid I shall exhaust your patience unless I proceed to this matter at once.

The property of metallic iron of purifying impure water has been known for a long time. It was found that, if ships had to take in foul water, its quality was being materially improved by being kept in iron tanks for a sufficient length of time. Here the rolling of the ship, constantly renewing the contact between the iron and water, accelerates the purifying action, which on land in a state of rest is extremely slow. Dr. Medlock first proposed, in 1857, to employ iron as a purifying medium by suspending, in a tank or reservoir containing the water to be purified by means of iron rods passing across it, iron wire of about $\frac{1}{16}$ of an inch in diameter, loosely packed in bundles or coils, in the proportion of about 1 lb. by weight of wire to every 100 gallons of water. The water was to be left in contact with the iron wire for 24 to 48 hours. Accepting the latter figure, a simple calculation tells us that for the London supply alone about 1,120 tons of such wire would be required. This enormous quantity would obviously have to be made for the purpose, and would require not unfrequent renewal, consequent to the corrosion of such fine wire. Moreover, the contact offered even by such a quantity appears insufficient, for, after doubling the proportion of iron wire proposed by Medlock, and leaving it in contact with the water to be purified for 48 hours, I found the purifying action very trifling. If applied to filtration by Water Companies, the storing of the water supply for 48 hours would here again be another insuperable difficulty.

Dr. Medlock explains the action of iron by its converting the nitrogenous matter and ammonia present in water, into nitrous and nitric acid.

I have once before, in the Chemical Section of this Society, alluded to the form of iron which I have proposed for the purpose of chemically purifying impure water. This is the so-called spongy iron, or metallic iron, which has been reduced from an oxide without fusion, and which is hence in a loose spongy state. Spongy iron, of which I here exhibit a sample, is made at present from burnt ores which are the residue of Spanish or other pyrites, after extracting their sulphur in the manufacture of soda, and their copper by the wet chlorization process. This, as you are aware, is the extraction employed by the Tharsis Copper Company and many other works in the kingdom. At present, owing to the abnormal prices of coal and iron, the price of spongy iron is rather high, but under normal conditions it can be made at from £2, 5s. to £2, 10s. per ton. There are also other materials from which

I am confident spongy iron could be made more cheaply, and which in all probability would be even better suited for purifying water, than the spongy iron made from burnt ores.

One cubic foot of spongy iron, in the loose state in which it is best suited for purifying water weighs only 70 to 80 lbs. It is thus evident that, weight for weight, it must offer an incomparably greater surface to the water than iron in any other form. Besides, if we bear in mind certain properties of a spongy platinum—its oxidation and conversion into acetic acid of alcohol—the supposition is not far fetched that spongy iron may also possess properties differing from those of iron which has been fused.

I have experimentally investigated the following properties of spongy iron :—

1. It decomposes water—even distilled water—which has been previously boiled. The decomposition of water is more energetic at the boiling point.

2. It reduces nitric acid to ammonia. This was proved in the following manner.—One of Fresenius' tubulated cylinders for washing gases was filled with spongy iron, and the latter washed with pure distilled water free from ammonia. No ammonia could be detected in the wash-water by the Nessler reagent. Then a solution of saltpetre, containing 5 m. gr. of nitrogen per litre, and free from ammonia, was slowly filtered through the spongy iron at the rate of 1 volume of the fluid, equal to the volume of spongy iron in the filters, in 3 hours. The filtrate contained 1·4 m. gr. of nitrogen per litre in the state of ammonia, or 28 per cent. of the equivalent quantity of nitric acid contained in the liquid.

3. From the fact that organic nitrogen and albuminoid ammonia are always much reduced after filtration through spongy iron, the conclusion must be drawn that spongy iron is capable of decomposing nitrogenous organic matter. Also the organic carbon, as will be shewn presently, is very considerably reduced by filtration through spongy iron.

4. A minute, almost constant quantity of iron, about 10 m. gr. per litre, is dissolved by means of the carbonic acid contained in water, when filtering it through spongy iron, forming ferrous carbonite. The latter is very soon oxidized and precipitated, and it may then be so completely separated from water by mechanical filtration through paper or sand, or by allowing it to subside, that potassium ferricyanide no longer indicates a trace of it.

If the water containing iron in solution be made to pass through a

layer of finely divided marble, or ordinary limestone, the iron is retained at once as ferric-hydrate.

The Loch Katrine water exhibits a peculiar and quite exceptional behaviour when filtered through spongy iron. If it be filtered very slowly, it comes out quite clear; but if filtered at the ordinary rate of other water, it turns yellowish, without depositing a precipitate of iron even after months. On adding nitric acid, the water, of which I here exhibit a sample, becomes colourless; ammonia reproduces the yellowish colour, and deposits, after some time, a small quantity of ferric-hydrate. Sodium carbonate renders the liquid opalescent, but produces no precipitate, even after several days' standing, whilst the liquid becomes colourless by filtration through finely pulverized marble. One drop of a very weak solution of potassium permanganate produces a red colouration permanent for at least half an hour. Some carbonic acid, passed into the yellow liquid, produces, in the course of a few hours, a precipitate of ferric-hydrate, and it becomes perfectly colourless. The same effect is produced by passing a moderate quantity of carbonic acid into the Loch Katrine water before filtering it through spongy iron. Potassium sulphocyanide, as might be expected, does not produce a red colouration, and ammonium sulphide does not indicate the presence of iron, unless the solution be boiled with a few drops of nitric acid previous to the addition of ammonium sulphide. Pure distilled water, on being filtered through spongy iron, neither dissolves iron nor turns yellowish.

It is clear that this yellow colouration is due to some ferrous compound formed if the quantity of carbonic acid in water be insufficient; but I am at a loss to name any known compound which would exhibit the above behaviour, especially one from which the iron would be precipitated by carbonic acid, which is, as a rule, a solvent of iron. Under ordinary conditions the water passes through the filter perfectly colourless, and only becomes opalescent when the iron begins to be precipitated; the precipitation of iron is retarded by passing an excess of carbonic acid into the water before filtration. The quantity of iron dissolved is of practical interest as regards the purification, inasmuch as the purification is more energetic, if by passing carbonic acid into a water the quantity of iron dissolved be increased. I have proved this by direct experiments.

5. The purification of water increases slightly for some five or six hours after the filtration through spongy iron has been completed.

6. If the iron be prevented from dissolving by adding to the water before filtration a minute quantity of sodium carbonate, the purifying power of spongy iron is considerably diminished.

7. The purifying action of spongy iron on organic matter is more energetic in hot weather than in winter, when the temperature of the water is frequently below the point (5° or 8°) at which fermentation ceases almost completely. These points appear to confirm my opinion, expressed on former occasions, that the action of spongy iron on impure water is two-fold, namely, chemical and mechanical. The chemical action must be found in its decomposition of water, to which is probably owing, in part at least, the decomposition of nitrates and nitrites, and the direct combination of nascent hydrogen with the nitrogen, to form ammonia. The readiest explanation for the decomposition of water is the intimate contact between electro-positive and electro-negative bodies, such as metallic iron and carbon, or even metallic iron and any ferric-oxide which has escaped reduction; and it may be well supposed that, consequent to the galvanic current thus produced, the atmospheric oxygen dissolved in water is ozonized, and caused to act as a powerful oxidizing agent. I am driven to this conclusion from the results of a large number of analyses made before and after filtration through spongy iron, which, notwithstanding the fact of the reduction of nitrates and nitrites, also clearly indicates an oxidizing action shewn by the increase of nitrates after filtration. This increase is mostly considerable—up to twice the original quantity—whilst sometimes there is also a decrease. The quantity of albuminoid ammonia is, as already explained, considerably reduced through the filtration. There is no fixed rule for the increase or decrease of free ammonia before and after filtration, which appears to depend upon the predominance of certain agencies.

I have already alluded to the action of the iron which is dissolved in the purification of water. This action may either consist chemically in the iron being reduced by agency of organic impurities, whilst in some low state of oxidation to a still lower state—the organic impurities thus becoming oxidized. The iron thus reduced may again be oxidized either by means of the oxygen dissolved in water, or the oxygen resulting from the decomposition of water; or the action of the dissolved iron may be either wholly or partly mechanical, consisting in a surface attraction of dissolved organic matter by the floculent precipitate of ferric-hydrate, similar to the action of the precipitated calcium carbonate in Clarks' softening process, of alumina and other bodies. That the ferric-hydrate has partly at least such a mechanical action, becomes probable from the fact that if its precipitation be hurried by boiling, the purifying action is lessened.

I regret that I have to offer several hypothetical explanations

regarding the theory of the action, but, although further researches may throw more light upon the question, it is well intelligible that the action of such complicated bodies upon each other, some of which are present only in extremely minute quantities, should be enveloped in some mystery.

There is, besides organic matter, another dangerous substance not unfrequently met with in water, viz., lead. You will recollect that this question has at various times been the subject of enquiry to a number of scientific and practical gentlemen, and as you will presently see that it is for us locally of interest, I hope you will allow me a few remarks upon it.

In 1851, Graham, Miller, and Hofmann, stated in their Report on the Chemical quality of the supply of water to the Metropolis, that they had arrived at the conclusion, that softness of water alone is not itself to any great extent the cause of lead being dissolved from lead pipes. According to these gentlemen, supported also by researches made by Dr. Medlock at a later date, this is caused chiefly if not wholly by nitrates contained in water, which may either be lixiviated from rocks or be the product of decomposition of organic matter. The absence of calcium carbonate dissolved in free carbonic acid in water deprives the latter, in their opinion, of one of readiest preventives of the solution of lead.

J. Smith found, in 1857, that as soon as the lead has begun to be corroded, or if it be exposed alternately to water and air, the corroding action of water is increased.

Pettenkofer experimented in 1864 on this question, and arrived at the conclusion that lead is oxidized exclusively by the action of atmospheric oxygen dissolved in water. Hence he shares Smith's views, that an alternate or simultaneous exposure to water and air is the most favourable condition for the corrosion of lead pipes. He is further of opinion, that the frequently observed action of soft water on lead pipes is due to the absorption of oxygen by rain water. To the same cause may be due the action of *soft* river water, whilst hard water, which, when taken up mineral matter loses more or less of its oxygen through the oxidation of organic and other matter, has a less corroding action.

Dr. Cr. Calvert communicated to the Meeting of the British Association, in 1861, the result of some 300 samples of Manchester water which he tested during a period of more than twelve months. His conclusions are, that the corroding action on lead pipes is strongest during the first four days, and that it then gradually decreases, unless the water be allowed to *stand* in pipes, or unless

lead pipes be for a time in disuse and then again employed. He found that soft water, which is almost free from organic matter, may also under certain circumstances corrode lead pipes to a great extent.

It is well known that in 1862 H. Schwartz proposed to put a stop to the corrosion of lead pipes, by converting their interior superficially into lead sulphide, by means of a hot cancentrated solution of sodium sulphide. He stated that not the least trace of lead is dissolved from pipes thus prepared, even when distilled water is allowed to stand in them for four weeks. I am not aware of any practical experiments with Schwart's plan, and it would at any rate be a great difficulty to remedy the corrosion, as the lead pipes already laid would have to be removed, which appears hardly practicable.

I would not have entered into these details had I not convinced myself, by an experiment, that our Loch Katrine water may also under favourable circumstances dissolve considerable quantities of lead. I allowed two gallons of Loch Katrine water to be in contact for 48 hours in a large evaporating basin with two pieces of old lead tubing. The contact under these conditions was certainly not nearly as large as it is in lead pipes of even large diameter, but I found that very nearly $2\frac{1}{4}$ milligrams of lead per litre, or 0·175 grains per gallon were dissolved. The lead could easily be estimated without concentration, by adding to the water after filtration through paper, a drop or two of hydrochloric acid and then some sulphuretted hydrogen water, as proposed by Persoz. It was then compared with a standard solution containing a known quantity of lead.

I filtered the water which contained the lead in solution through one of the spongy iron filters. In the filtered water no trace of a colouring could be detected as above, and even after concentration to $\frac{1}{30}$ of its volume, the presence of lead could not be detected. This result was to be expected from the well-known fact, that lead salts are precipitated by metallic iron, and that spongy iron is a much more energetic precipitant than iron in other forms. This property, too, is an important and welcome addition to the employment of spongy iron for purifying water.

Allow me, in concluding, a few words about the practical application of spongy iron to the purification of water.

One of the most important questions in regard to any filtering material is, how long will it remain active, and when does it require to be renewed? In answering this question, I prefer to omit laboratory experiments, but I may mention that in London, at the Water Works of the Southwark and Vauxhall Company, an experimental

spongy iron filter has been at work since the end of October last, which has an area of 50 square feet, and a thickness of spongy iron of 1 foot. The water is calculated to pass through this filter at the rate of 300 cubic feet an hour, but I understand this supply is somewhat deficient, owing to the size of the supply pipe. The last sample of filtered and unfiltered water was sent to me when the meter indicated 100,000 cubic feet of water, or 2,000 times the volume of spongy iron in the filter, I then found the purification, notwithstanding the unfavourable action of the cold weather, equal to that obtained when first starting the filter.

Of course these 2,000 volumes give nothing like a practical result if the spongy iron is to be used by Water Companies, but for the reasons before explained, I would prefer to see it used in small domestic filters, and in this case, if such a filter were to contain one gallon by volume of spongy iron (or 10 to 12 lbs. by weight), a yield of 2,000 gallons of purified water with an undiminished action of the spongy iron would more approach a practical result. Supposing 10 gallons of water a day for cooking and drinking, such a domestic filter would, as far as the action of the spongy iron is concerned, purify as energetically after 200 days as on the first day, so, that doing away with all hypothesis, it would certainly not require a renewal under less than six months of constant use, and at a cost of hardly one shilling. Of course, in such domestic filters the filtration can be made much slower in order to effect a more complete purification than would be practicable for Water Companies. I should think that one gallon in half-an-hour, or even in an hour, is a sufficient speed for domestic filters.

I have been ably assisted by Messrs. Murray & Co., of the Caledonian Pottery, Rutherglen, in the construction of such domestic filters according to my design.

The spongy iron through which the water is to be filtered or purified first passes, is contained in a stoneware vessel with a slightly curved bottom. On the top of this bottom there is a perforated bottom on which the spongy iron rests. An opening in the curved bottom is connected with an earthenware pipe which passes up the outer side of the vessel to slightly above the level of the spongy iron. Here the pipe communicates with another pipe, which passes from the top of the outside of the spongy iron vessel down to the centre of its closed bottom. The latter pipe is open at the top and bottom, for if it were closed at the top it would act as a syphon and draw off the water from the spongy iron, which should as far as possible always remain covered with water. An alternate exposure

to air and water causes the spongy iron to be oxidized, when it loses more or less its purifying power. A screw tap at the lower end of the latter pipe serves to regulate the flow of the water through the spongy iron.

The spongy iron vessel is placed in the casting of an ordinary stoneware filter with perforated bottom, beneath which there is a reservoir for the filtered water. On the top of the perforated bottom is placed a layer, some four inches thick, of finely divided marble or limestone, on to which the water, which contains the iron in solution, flows from the screw tap. By action of the limestone the water is readily freed from iron and collects in the reservoir ready for use.

The whole filter is covered with a lid as usual.

The layer of spongy iron is about eight inches thick, and I prefer having six inches of a rather coarse and two of finer spongy iron on the top, in order that the latter may also act as a mechanical strainer retaining suspended impurities. If the water contains much suspended matter, the finer spongy iron may from time to time be renewed.

The following analytical results were obtained in my laboratory when filtering water, slightly contaminated with sewage, through Spencer's Magnetic Carbide Filter, Dahlke's Silicated Carbon Filter, and the Spongy Iron Filter. All these filters were well washed with the filtered water before collecting the sample, the latter was analyzed immediately, as it was found that the water filtered through the Spongy Iron Filter improves on standing. The latter filter yielded 1 gallon in 45 minutes, the other filters, being of smaller size, 1 gallon in 90 minutes, as far as this was practicable, owing to their liability to become choked.

Description of Water.	Free Ammonia.		Albuminoid Ammonia.		Organic Carbon.		Organic Nitrogen.		Total comb. Nitrogen.	
	Found.*	†	Found.*	†	Found*.	†	Found.*	†	Found.*	†
Unfiltered Water, . .	0·0694	100	0·0225	100	0·2499	100	0·0843	100	0·1414	100
Same filtered through Spencer's Filter, .	0·0502	82	0·0108	48	0·1045	42	0·0431	51	0·0927	65
Same filtered through Dahlke's Filter, .	0·0389	56	0·0102	45	0·0558	22	0·0764	90	0·1084	76
Same filtered through Spongy Iron Filter, .	0·0522	75	0·0056	25	0·0216	10	0·0216	26	0·0646	46

* In 100,000 parts of water.

† This column shews the percentage still contained in the filtered water, the unfiltered water = 100.

I have here a microscopic preparation of the unfiltered water, and of the same water after filtering through spongy iron. The samples were obtained by evaporation at a low temperature—a method

explained by me at the meeting of the British Association at Edinburgh. The preparations speak for themselves, shewing an unmistakable purification even to the unitiated eye. I classify the sample filtered through spongy iron from my microscopic examination as a water of a very fair quality.*

I expect there are some medical gentlemen here present, and I wish just to say a few words about another application of spongy iron. The apparatus here before you consists of a spongy iron filter through which water, highly charged with carbonic acid, passes from the Seltzogene. During a visit to some of the German Iron and Steel Waters, the idea suggested itself to me, that the carbonated iron water thus produced, instead of the natural water, might be employed in some cases with advantage, and that the iron in this form may be preferable even to the ordinary preparations generally used.† Some medical gentlemen, to whom I mentioned my idea, told me that artificial ferrous carbonate had never been used internally before, but that they thought the matter well worth trying. I omit the observations which I have made myself in reference to the effect of my preparation, and beg leave to conclude with a report kindly sent to me by Dr. Thomas, of the Royal Infirmary, to whom I gave such an apparatus about the beginning of December.

Dr. Thomas writes:

"Your carbonated iron water has been used in five cases, the quantity allowed being a tumbler full three times a day. Some of the cases were phthisical patients with a considerable degree of stomach derangement, indicated by nausea and occasional vomiting. They were also very anaemic. I desired the patients at the time they commenced the treatment to watch the effect closely, and at the present date they all state, without the slightest hint from me, that now the stomach gives them no trouble, that they have a great relish for food, and feel stronger. The colour has also improved, but this may be owing to the better diet.

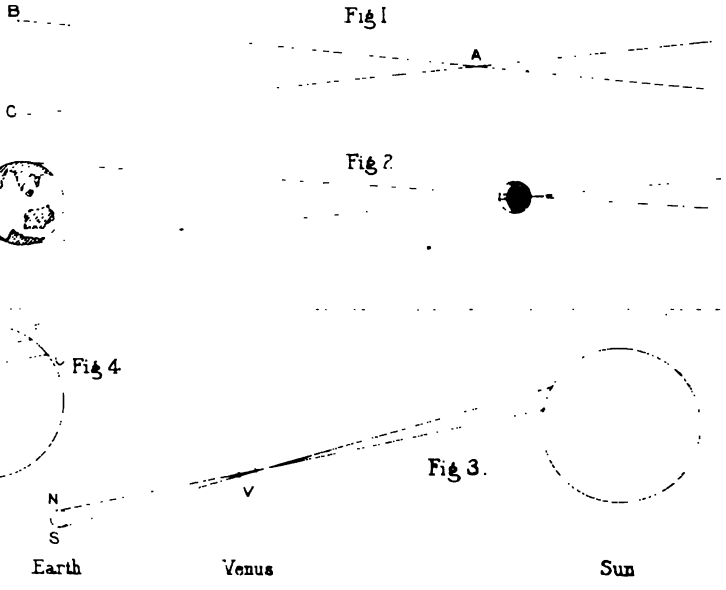
"The fourth case I tried it in was one of chronic vomiting, and it entirely failed to mitigate the symptoms in the least. The fifth case was one of pure anaemic in a hysterical girl. It was the only treatment adopted except a little potassium bromide, and she gathered flesh, and acquired a better complexion, and this more especially after the potassium bromide was stopped owing to its bringing out a rash.

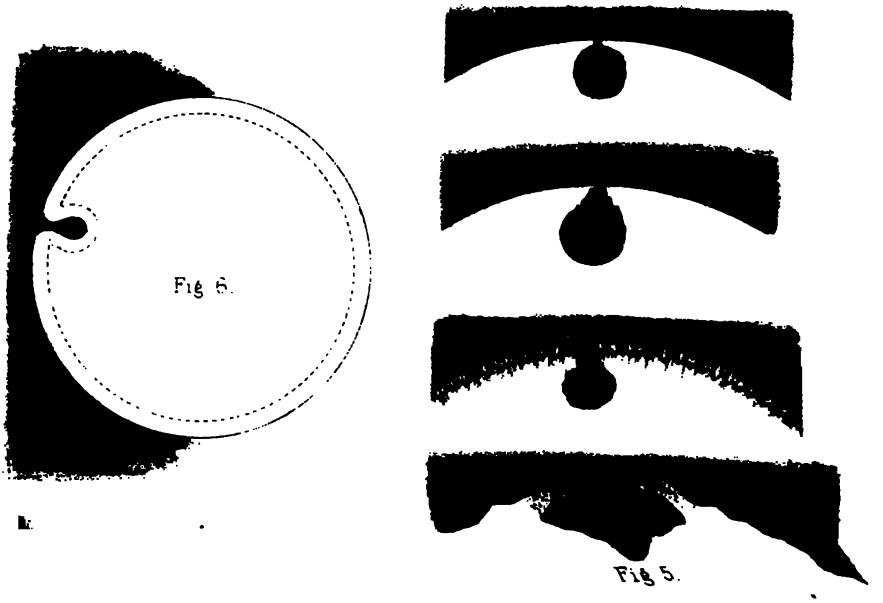
* The latter sample exhibited only well defined crystals of calcium carbonate, whilst the unfiltered water exhibited no such crystals, but those organisms which always accompany fermentation.

† A similar suggestion was made several years ago by Von Hauer.

Proceedings of the Philosophical Society of Glasgow, 1872-73.

THE TRANSIT OF VENUS IN 1874.





I fancied also that it modified the diarrhœa in one of the cases very considerably. No other drugs were used in the cases during the trial. But the trial has been far too limited to afford any definite grounds for drawing inferences as to the effects, and it would require much more extended observations before any definite conclusion could be legitimately laid down. Still the results, as far as they went, seemed very favourable."

Thus far Dr. Thomas. The apparatus is inexpensive, and any one can put it together, excluding the Seltzogene, for a couple of shillings. It is thus easily accessible even to poor people.

XVIII.—*The Transit of Venus in 1874.*—*By* PROFESSOR G. FORBES.*

Read before the Society, March 19, 1873.

THERE is no problem that has been so universal an object of interest to the students of science in all ages as the solving of the mystery of the Solar System. The proportions of this structure are tolerably well known to us. It would be a difficult matter to measure directly the relative distances of the planets from the Sun. But Kepler's third law removes the necessity for this. That law tells us the connection which exists between a planet's period of revolution and its distance from the Sun. It is a comparatively easy thing to find out the period of a planet's revolution, and that being known, we know its distance from the Sun compared with that of the Earth. If, then, we knew the Earth's distance from the Sun in miles, we should know the distances of all the other planets from the Sun in miles.

Seeing, then, how important is a knowledge of this distance, it is not to be wondered at that the most energetic preparations are now being made to take advantage of the most favourable opportunity for the purpose that has ever occurred; and it is to be my pleasure to explain to you this evening what steps are being taken to make use of this grand opportunity to the best purpose.

The principle employed in this observation is the same as that used by surveyors in finding the distance of some remote object. Let A (Pl. I. fig. 1) be the remote object. The surveyor chooses two spots, B C, whose distance he measures. Suppose it to be one

* I have attempted, in these pages, to give among other things a faithful history of our knowledge of the Solar Parallax. I have examined every memoir or article to which allusion has been made, except the *Voyage en Californe*, and *Littrow's Account of the Wardhus Observations*.—G. F.

mile. He draws this distance equal, say, to one inch upon a sheet of paper. He then takes a telescope, mounted so as to enable him to measure any angle through which it is turned. He places the telescope at B, pointing towards C, and then, having turned it round till it points to A, he knows what the angle at B is. He draws the line, B A, in the proper direction on his paper, and he knows that the remote object lies somewhere on the line B A. He then does the same with C, and thus he knows that the remote object lies upon the line C A, but A is the only point in both the lines C A and B A. Hence A corresponds to the distant object. If on measuring C A he finds it to be 30 inches, then, since C B, which is one inch, means one mile, C A, which is 30 inches, means 30 miles, and this is what he wanted to find out.

If, instead of taking a base-line (as it is called) of one mile, the diameter of the Earth, or 8,000 miles, were chosen; then, if the Moon be the distant object, we can determine its distance (fig. 2). This is exactly the manner in which that problem has been solved.

It might be thought to be an easy thing to do the same thing for the Sun. But a practical difficulty here suggests itself. The Sun being at an enormously greater distance from us than the Moon, it is clear that two observers at opposite sides of the Earth's diameter will be pointing their telescopes almost exactly in the same direction. If we draw two lines from the distant object, one to the centre of the Earth, the other to an observer on the surface of the Earth, the greater the angle between these two lines the more easy is it to measure the distance. This angle is called the *parallax* of a heavenly body.

But although we cannot apply the method I have described to finding the distance of the Sun directly, still we can do so in another manner. We know the *relative* distances of all the planets from the Sun and from each other. If, then, we can find the distance of any one of the planets from the Earth in miles, we shall have the means of determining the distances of all the planets from the Sun. Now, Venus and Mars are two planets that come at certain times closer than any other planet to the Earth. But, unfortunately, when Venus is closest to the Earth she is generally invisible, because the whole of her illuminated side is turned away from us. Mars, however, is a planet that gives us a most favourable opportunity for determining its distance. The advantage is increased by this peculiarity, that every fifteen years Mars is at its shortest distance from the Sun at the same time that the Earth is at its greatest distance, the two planets being also in the same line with the Sun, so that they are closer than we might have thought possible.

This method has been utilized, and has given results agreeing with other methods, and inasmuch as this gives a favourable opportunity every 15 years, it is a method which will, in time, give a very accurate value of the planet's parallax, and hence of the Sun's parallax. In the year 1878 such an opportunity will occur that will be utilized by all observatories favourably situated.

This is the method that first gave to astronomers an approximate knowledge of the dimensions of the Solar System. The observation was made in 1671, as suggested by Cassini, and the parallax was found to be 9″·5, a great advance on former determinations that were given in minutes. Professor Henderson, in 1836, and Mr. Stone, in 1862, utilized this method.

It has been found that light takes a sensible time to propagate itself through space. Hence, when one of Jupiter's satellites passes into the shadow of the planet, this fact is not communicated to our vision for something like 38 minutes, the time taken by light to pass from Jupiter to the Earth. Now, when we are on the same side of the Sun as Jupiter, this distance is shorter by the whole diameter of the Earth's orbit than when we are at the opposite side of the Sun. Hence, in the former case, the eclipses will seem to take place sooner than the predicted time, and in the latter case later. The difference in either case is about 8 minutes, and as we know that light travels over 298,500 kilomètres per second,* this tells us that our distance from the Sun is about 91 million miles.

But our knowledge of the velocity of light has been utilized in another manner to solve the same problem. You see that if we know the Earth's velocity in miles, we can find its distance from the Sun. For if it goes over 1½ million miles in one day, it goes over 365 times that in a year, and that measures in miles the circumference of the Earth's orbit, and hence we can get our distance from the Sun. How then are we to find the velocity of the Earth in miles. This depends on a curious property of light. In a steady down-pour of rain you hold your umbrella upright if you are standing still, but incline it forward if you are walking fast. This is to make the umbrella catch the rain-drops. The amount of inclination you give it depends upon the rate at which you are walking compared with the velocity with which the drops fall. The same thing happens with light. We have to incline our telescopes forward a little, in the direction in which the earth is moving, to catch the rays of light; and at opposite seasons of the year the Earth is moving in contrary direc-

* As determined by Foucault, *Comptes Rendus de l' Acad. des Sciences*, lv., 502; also by Cornu, *Comptes Rendus*, 1873, Feb. 10.

tions, and the telescope has to be pointed in sensibly different directions. The inclination that a telescope receives is known, and the velocity of light being known, we can find the velocity of the Earth, and hence, as I have shewn, the distance of the Earth from the Sun.

There is another method of peculiar interest depending upon the motions of the Moon. The law of gravitation says that the attraction of each body for each other one depends upon the distance between them. The Moon is attracted to the Earth by a force, depending upon the distance of the Moon, which is known in miles. But the Moon is caused to deviate from its natural course on account of the Sun's attraction. This depends upon the distance of the Sun from the Earth, and if this be not known exactly in miles we shall see that it is impossible to apply calculation to foretell the motions of the Moon: for, if upon any scale we attempt to lay down upon paper the relative positions of the Sun, Earth, and Moon, we shall place the Moon at its proper distance, and the Sun, though in its proper direction will not be placed at the proper distance, and we shall not know the direction in which it attracts the Moon, nor the magnitude of this attraction, and we shall make our calculations wrongly, and the Moon's observed place will differ considerably from its calculated place.

Such a difference was actually detected by the illustrious Hansen, whose tables of the Moon are the best we possess. Hansen saw that this must be due to a wrong assumption as to the distance of the Sun, and communicated his doubts to the Astronomer-Royal* in the year 1854. This led to a re-discussion of our knowledge of the subject which has confirmed Hansen's views, and which leads us to see the importance of knowing accurately the Sun's distance, if we wish ever to have our tables of the Moon so accurate that we may determine the longitude by their aid. This method for investigating the Solar Parallax was first used by Laplace.†

More recently, M. Le Verrier has suggested a new method that promises in time to be the best.‡ In the Lunar Theory, an equation appears connecting the relative masses of the Earth and Sun with the Solar Parallax, so that if we know the one we can find the other; and from a peculiarity in the equations, a small error in determining the relative masses will affect only very slightly the deduced parallax. Le Verrier finds the ratio of the masses of the Earth and Sun by determining the effect of the Earth's attraction

* *Monthly Notices, R.A.S.* 1854, Nov. (Vol. XV.)

† *Système du Monde*, Tome ii., p. 91. ‡ *Comptes Rendus*, 1872, July 22.

upon Venus and Mars. This being applied to the Lunar Theory gives a value of the Solar Parallax.*

The method that is to occupy our attention to-night is another one depending upon the transit of Venus across the Sun's surface. This planet, as she proceeds through her different phases, is at very different distances from the Earth. When at the opposite side of the Sun she is about 158,000,000 miles away; but when between the Sun and us, only 25,000,000. No other planet ever comes so near us. This is evidently a good time for determining its distance from the Earth. But, unfortunately, when between us and the Sun, it has its illuminated side wholly turned away from us, and is consequently invisible, except when seen as a black spot upon the Sun's surface. This does not always occur on account of the inclination of the path of Venus to the path of the Earth, so that generally when at its nearest distance from the Earth it passes above or below the Sun and is not seen. It is only when the Earth and Venus happen to be at the same time in one of those two positions, where the planes of the two orbits cut each other, that the planet will be projected upon the Sun. Kepler, in 1629, predicted the possibility of such an occurrence for the years 1631 and 1639. The former was not visible from Europe, and astronomers, ascribing it to an error of Kepler's, did not watch for that of 1639. To two happy young Englishmen alone was the glorious sight revealed on this occasion. Jeremiah Horrox, who died when twenty-two years of age, and who was perhaps the most promising astronomer that ever lived, made preparations for its observation, and urged his friends to do the same. He has left us a drawing of the transit as he saw it, and a full account of the important observation. His friend, Crabtree, also witnessed the phenomenon, but was unable to make accurate observations. We cannot devote more time to this interesting observation, as it has no reference to the Sun's distance.

In 1663, the celebrated James Gregory, in his great work, *The Opitica Promota, prop.* 87, *Scholium*, alludes, in the clearest terms, to the possibility of determining the Sun's parallax by means of the transit of an inferior planet. He has been shewing methods of finding the parallax of a planet, by observers at different parts of the earth comparing the position of the planet with that of a star. He goes on to take in place of a fixed star another planet, the two being in conjunction. The application of this to the case of Mercury or Venus and the Sun was obvious.

* Observations of the Transits of Venus are valuable for this method.

But Halley was the first to see the accuracy of the method which depends on observations of contact of the planet with the Sun. He first mentions the method in a letter from St. Helena to Sir Jonas Moore, dated 1677,* when he had seen a transit of Mercury. Frequently afterwards he urged his countrymen to prepare to make this observation on the occasion of the transits that would occur in 1761 and 1769, when he should be dead.†

He even pointed out the best stations for observation. We may consider the earth to be at rest if we suppose Venus to move with the velocity she has relative to the Earth. He supposed that the planet would cross near the Sun's centre, and that the transit would occupy about eight hours. An observer in India would see the commencement of the transit four hours before mid-day, and the end of the transit four hours after mid-day. But, in the meantime, the part of the Earth where he is has been moving from west to east, and Venus has moved from east to west, hence the duration of transit will have been shortened. But at Hudson's Bay the transit begins just before sunset and ends just after sunrise, that part of the Earth having moved in mean time from east to west so as to lengthen the transit; and thus at one place the duration of transit is lengthened, and at the other shortened, and the difference of time depends upon the parallaxes of Venus and the Sun.‡

Halley's method is called that of durations. It has been applied by choosing stations on a principle differing slightly from that indicated by Halley. The method proposed by him can only be applied when Venus passes near to the Sun's centre. For if it do not, the transit will last so short a time that it will be impossible to find an accessible station when the commencement takes place just before sunset and the end just after sunrise. Halley was

* *Hooke's Lectures and Collections*, 1678.

† *Cat. Stell. Austr.*; also *Phil. Trans.*, 1694, 1716.

‡ This lengthening or shortening of the time of transit will be rendered more evident by an analogy. A person standing still sees a carriage pass between him and a distant house. The carriage will take a certain time to pass the house. But if he be also moving, and in the same direction with the carriage, the transit of the carriage will take longer; but if he move in the opposite direction to the carriage, the transit will take a shorter time. If, then, two persons be seated at opposite sides of a merry-go-round, so that at the time the carriage seems to be passing the distant house, one observer is moving with the carriage and the other in the opposite direction; then one observer will see the time lengthened, and the other shortened. Now, the world is such a merry-go-round, and the positions of these two people correspond to the positions of India and Hudson's Bay, as pointed out by Halley.

in this misled by the tables of Venus; further, he made a small but important error of calculation, so that at Hudson's Bay, his northern station, the transit was invisible.

The modification of Halley's method is this:—The path that Venus seems to describe in crossing the Sun will depend upon the position of the observer. As shewn in figs. 3 and 4, it will be A B C or D E F, according as the observer is at the north or south of the globe. But it is clear that if the transit take place along A B C, it will begin earlier and end later than if it was along D E F; and the duration of the transit will be shorter. The difference of durations will depend upon the distances of A B C and D E F from the Sun's centre, *i.e.*, upon the angle N V S. This angle being known, and the distance N S being known in miles, the easiest geometry gives the distance from the Earth to Venus, and hence to the Sun.

Transits of Venus take place in pairs, eight years apart, with an interval of more than a century between two pairs. The transits of three centuries are as follows:—

1631. Dec. Predicted by Kepler, but not observed.
1639. Dec. Observed by Horrox and Crabtree.
1761. June. } Alluded to by Halley, and observed by the English
1769. June. } and French.
1874. Dec. } To be observed by the British, French, German,
1882. Dec. } Russian, American, and Portuguese (?) nations,* and by private expeditions.

The transits always take place at mid-summer or mid-winter. Halley took advantage of this in proposing his method, which would otherwise be inapplicable.

In 1761, the English were represented by Messrs. Mason and Dixon, at the Cape of Good Hope, and the French by the celebrated Pingré, at the island of Rodriguez. A host of observers observed the phenomenon from northern regions. Unfortunately, at scarcely a single station was the transit seen completely. Hence the method of durations could not be applied, and another, originally proposed by De l'Isle,† came into use. This takes advantage of the principle that the ingress will take place later when seen from some parts of the Earth than from other parts, as explained above. So with the egress of the planet from the Sun's disc.

* The Government of New South Wales has also voted £1,000 for three stations in Australia.

† *Histoire de l'Acad. des Sciences*, 1761, p. 112.

Hence if the absolute time of contact of Venus with the Sun's edge be observed at the two places, the difference in time will be a measure of Venus' parallax. This is called the method of absolute time differences. To apply it we must know the difference in the longitudes of the two places. This is even now a matter of some difficulty, and was still more so in 1761. Hence the application of this method was not successful, and the results of that transit were unsatisfactory.

Not daunted by the comparative failure of this attempt, the astronomers of the last century made vigorous efforts to render the transit of 1769 successful. The transit of 1761 was utilized in so far as it pointed out the difficulties of this kind of observation, and gave them an approximate value of the Sun's parallax to help them in choosing the most advantageous stations from which to observe the next transit.

Halley had no conception, when he proposed this kind of observation, of the difficulties attending it. The values which have been deduced from the observations of last century, and especially of the year 1761, have varied considerably according to the mode of reducing the observations. Thus, in 1761, Lalande* found, from the observations of Pingré, 9″·4 for the Solar Parallax; while Maskelyne found, from the work of Mason and Dixon, 8″·6. Short† made it 8″·65, Wargentin,‡ 8″·1 to 8″·3.

Encke shewed§ that the differences were partly due to an error in the longitude of Rodriguez. This question will be capable of further discussion after 1874, as Rodriguez is one of the stations chosen by the English on that occasion.

Even after Encke had discussed with all possible care the observations both of 1761 and 1769, and had found a close agreement between the values deduced from the two transits‖—even then, I say, there were doubts raised as to the correctness of the quantity thus found. The reasons of these doubts were manifold. In the first place, in order to get any value whatever for the Solar Parallax, Encke had to assume that enormous errors of observation had been committed by some of the observers; and, again, all the other methods which we have spoken of were found to give accordant values for the Solar Parallax, but values that differed considerably from Encke's determination.

It was with no small satisfaction, then, that astronomers learnt

* *Phil. Trans.*, vol. lii., p. 647.
† *Ibid.*, p. 648.
‡ *Ibid.*, p. 657.
§ *Zach. Corresp.* II., 1819, p. 367.
Berlin Abhandlungen, 1835, pp. 295–310.

that M. Powalky, in 1864, had deduced a sensibly greater value for the Solar Parallax by using more accurate values for the longitudes of the places of observation.

But Mr. Stone, Her Majesty's astronomer at the Cape of Good Hope, has lately discussed anew these observations.* He finds that when the remarks of the observers are rightly interpreted, all the observations agree without any extravagant errors of observation; and, moreover, the value of the Solar Parallax thus deduced agrees with the values found by other means. Mr. Stone deserves the thanks of the scientific world for having convinced them that this method, which at one time was falling into disrepute, is really one of the most trustworthy.

The uncertainty of observation which Mr. Stone cleared away is one of a very curious optical character. It is found that Venus, at the time when she is almost exactly within the edge of the Sun does not retain her round aspect, but becomes pear-shaped, in such a manner that a black drop or ligament seems to connect her to the limb of the Sun. This ligament sometimes appears simply as a fine black thread, connecting the planet with the limb of the Sun. I will not stop to quote descriptions of these appearances. They are shewn in fig. 5, and descriptions may be read without limit by referring to the *Philosophical Transactions* for 1769-70.† Sometimes a ring of light was seen to surround the planet.

This is no fancy of the observers. It has been noticed by too many to allow of such an explanation. More especially was it observed in the case of a transit of Mercury that occurred in 1868.‡ It is found that there are two perfectly distinct phases in connection with the appearance. If the planet be entering upon the limb of the Sun, the first phase is one where the edges of the Sun and planet *seem* to be in contact. The second is at the instant when the black drop breaks off and a flood of light sweeps in between the planet and the Sun. This occurs very suddenly, and, as we shall presently see, is the true criterion of almost perfect contact.

In the case of the above transit of Mercury, studied by six experienced observers at Greenwich Observatory, two most curious facts appear. Firstly, the times of contact vary with the different

* *Monthly Notices of the R.A.S.*, xxviii., p. 155.

† The drawings in figure 5 are taken from the originals by Bevis, Hirst, Bayley, and Mayer respectively, in the *Phil. Trans.* Professor Grant states that the last one resembles the appearance of Mercury, as seen in the transit of 1868 from Glasgow Observatory, the Sun being nearly on the horizon.

‡ *Monthly Notices of the R.A.S.*, xxix., p. 17, &c.

observers to the extent of 13½ seconds. And secondly, the shape of the planet, when the ligament was just formed, varied considerably with different observers.

As to the cause of this phenomenon Lalande* justly ascribes it to irradiation. Irradiation is that curious property of the eye in virtue of which a star or anything bright appears larger than it really is. In this way the Sun's diameter seems increased. The Sun's light also encroaches upon the disc of the planet, and makes it seem smaller than it really is. But when Venus and the Sun have their edges just in contact (as shewn by the dotted line in fig 6), then there is no light at that point which can encroach. Hence we see at this point the full extent of Venus, and she seems to be connected with the planet by a black drop.

What the cause of irradiation is, is quite another matter. The question has been taken up by people under such false ideas as to the nature of the problem, that the whole matter has become so much involved as to necessitate one's commencing afresh. This I have been attempting during the past winter, and hope soon to publish the results. The enormous differences in time between the determinations of contact by different observers have caused many astronomers to despair of ever perfecting this method of the transits of Venus. It is perhaps possible to remove this difficulty, at least in part. As to the cause of irradiation, I may state that I have found it is *not* due to the spherical aberration of the eye, as some have supposed, nor to an extension over a sensible portion of the retina of an impression received, as others have imagined.

It was an error made by Encke in interpreting the language of the observers that caused him to get an erroneous result. The observations made use of by Encke and Stone are the same. First, there are those by Father Hell and Sajnovics, made at Wardhus, in Lapland,† then, again, those of Messrs. Wales and Dymond,‡ at Kola, in Hudson's Bay; then those made by Chappe, Pauly, Vicente Doz, and Salvador Medina, at St. Joseph,§ in California; and, lastly, those of Charles Green and Captain Cook at Otaheite,‖ then called St. George's Isle. The two terms, apparent contact and

* *Mems. Acad. des Sciences*, 1770, p. 409.

† *Reise nach Wardoe bei Lapland, und Seine Beobachtung des Venus Durchganges*, im Jahr, 1769. Carl Ludwig Littrow. Wien, 1835.

‡ *Phil. Trans.*, 1769.

§ *Voyage en Californe pour l'observation du passage de Venus sur le disque du Soliel*, le Juin, 1769. Par M. Chappe. Paris, 1772.

‖ *Phil. Trans.*, 1771.

real contact, occur continually in these accounts, and the wording of the phrases requires some care to arrive at a correct interpretation of them. This is what Stone did.

It appears, then, that we have determinations of the Solar Parallax derived from several methods which differ in their degree of accuracy.

In the annexed Table these results are grouped together for comparison :—

Method.	Parallax.	Dist. of Sun in miles.	Computer.
Transit of Venus,* .	8″·91	91,580,000	Stone.
Opposition of Mars,† .	8″·943	91,240,000	Stone.
Lunar Theory,‡ . .	8″·916	91,520,000	Hansen.
Lunar Theory, § . .	8″·850	92,200,000	Stone.
Planetary Theory,‖ .	8″·859	92,110,000	Le Verrier.
Jupiter's Satellites and Velocity of Light,¶	8″·86	92,100,000	Loucault.
Constant of Aberration & Velocity of Light,**	8″·86	92,100,000	Cornu.

Although these results agree together very well, it is highly important to get more certain results. This is why so great trouble is being taken to make the most of the coming transits of Venus. We, natives of Britain, are bound to do this were it only to vindicate the opinion of Halley, who placed such value upon this method.

The distance of the Sun, as determined by different methods, is found to vary to the extent of millions of miles. Some may be disposed to ask, What is a million of miles to an astronomer, or a million of years to a geologist? In reply to this it must be borne in mind that the important problems of the prediction of the Moon's and Planets' places cannot be solved with any great accuracy until the Solar Parallax be known exactly. And, although our means are hardly sufficient at present to advance much farther in improving the planetary tables, still we must not forget that future astronomers, for the next 130 years, will be, in a great measure, dependent upon our exertions during the next few years for the determination of this constant.

Before proceeding to detail the preparations that have already been made by different nations and individuals, it will be well to point out another means of utilizing the transit.

* *Monthly Notices*, xxviii., 255.
† *Ibid.*, xxiii., 183.
‡ *Ibid.*, xxiv., 8.
§ *Ibid.*, xxvii., 271.
‖ *Comptes Rendus*, 1872, July 22.
¶ *Ibid.*, 1862, p. 502.
** *Ibid.*, 1873, p. 341.

We have already seen that at some parts of the Earth's surface the duration of transit is less than at others. An observer in Southern latitudes will see the planet cross along the line D F, but in Northern latitudes, along A C (fig. 4). The reason of the difference of durations is, that Venus seems to pass closer to the Sun's centre from one station than from the other. If, then, we can determine with great exactness the distance between the centres of the Sun and Venus, as seen from different places, we are measuring simply the parallax of Venus. This method was used by Lalande in 1761.

The instrument employed for this is called a heliometer, because it was originally devised for measuring the diameter of the Sun. It consists of a common telescope mounted much in the usual way, but there is this peculiarity, that the object-glass of the telescope is cut in two, and the two halves can be slid past each other in such a manner as to give us two images of the Sun. In measuring the diameter of the Sun these two halves are slid past each other until the two images of the Sun are seen just to overlap. A scale shews how far the two portions of the lens have been separated, and this measures the angular diameter of the Sun.

In using this instrument for observing the transit of Venus, it is proposed to measure the distance of each edge of Venus from each edge of the Sun, and also to measure the diameter of the Sun in the manner described above, and the diameter of Venus in the same way. This being done repeatedly, we shall have every datum necessary for determining the distance of the centres of Venus and the Sun at different periods in the transit, and each of these measures, combined with the time of observation, will give us the means of calculating the least angular distance between the centres of Venus and the Sun; this being done for two stations, at opposite sides of the Earth, we can find the difference between the least distances thus determined. This difference measures simply the parallax of Venus.

The method of employing the heliometer has been most ably discussed by Lord Lindsay and Mr. Gill in the *Monthly Notices of the R.A.S.** For my own part, it seems that if this be one of the most accurate means of determining the Solar Parallax, then the transit of Venus has little or no superiority over an opposition of Mars in that respect. For the parallaxes of Venus and Mars are in the two cases in the ratio of 26 to 33, and we are here measuring only the *difference* between Venus' parallax and the Sun's.

* Nov., 1872.

To observe the former only a few hours are allowed; but in the latter, there is time enough to make hundreds of observations. And the position of Mars with respect to a star can be as well determined as that of Venus with respect to the centre of the Sun can by means of the heliometer.

But there is still another means of making the same observation, a means that promises to be of great value. I refer to the application of photography. It is not generally known how extremely accurate a means of observation photography is. We owe much to Mr. De la Rue,* whose success in the application of photography to astronomy has been unequalled, for having given us a most succinct account of what has been done in this way. The method has been employed in America to measure the distances between double stars. The double star is photographed, and the distance is then measured as accurately as possible. Professor Bond finds that the probable error of such a distance is $\pm 0''{\cdot}072$, or $\frac{1}{5}$ of the probable error of a similar measure made with a filar micrometer, as estimated by Struve. Photographic pictures of the Sun have, during many years, been taken daily at Kew, and it is found that an extremely accurate measure of the Sun's diameter may thus be made.

A telescope has to be made specially for this purpose, to give a good image. Such an instrument is called a photoheliograph. If the Kew photoheliograph be employed upon the transit of Venus, the Sun would have a semi-diameter = 1·9658 inches and Venus of 0·06333 inches. The maximum possible displacement would be 0·0957 inches. That would be for two places, at opposite sides of the Earth's diameter, when the Sun is therefore on the horizon. It may be well to remark that this is a position in which, so far as past experience goes, it is hopeless to get a photograph.

This method has many advantages. The instant of observation is so determinate, compared with the heliometer. Then, again, the actual measurements are made afterwards when there is no fear of the excitement of the moment carrying one away. The measurements may be repeated as often as is desirable.

But there are difficulties. The wet collodion process offers some objections. Some photographers have tried to use dry processes, in which the developing of the picture can be done at leisure. Professor Vögel, in Holstein, and Colonel Smyśloff, at Wilna, have succeeded in taking pictures by dry processes quite instantaneously and delicately. A great advantage of this is that there can be no shrinking of the film of collodion. This is a point that

* Address to the Math. and Phys. Section of the British Association, Brighton, 1872.

has bothered photographers a good deal. The Germans (and especially Herr Paschen) have made a number of experiments on the subject.* So has Mr. De la Rue. The result appears to be that any shrinkage takes place only in the thickness of the film. It is a very great pity that no one seems to have undertaken to test the utility of the Daguerreotype. The advantages would be considerable. The delicacy is greater than can be obtained by any other method. There can be no shrinking. The curvature of the field of view of the telescope may even be allowed for, by having a sensitive plate of spherical form. The spreading of the bright parts of the picture by photographic irradiation is reduced to a minimum. The only disadvantage is, that we cannot print a copy from it. But it is not likely that we should trust to measurements made on a printed copy even from a glass negative.

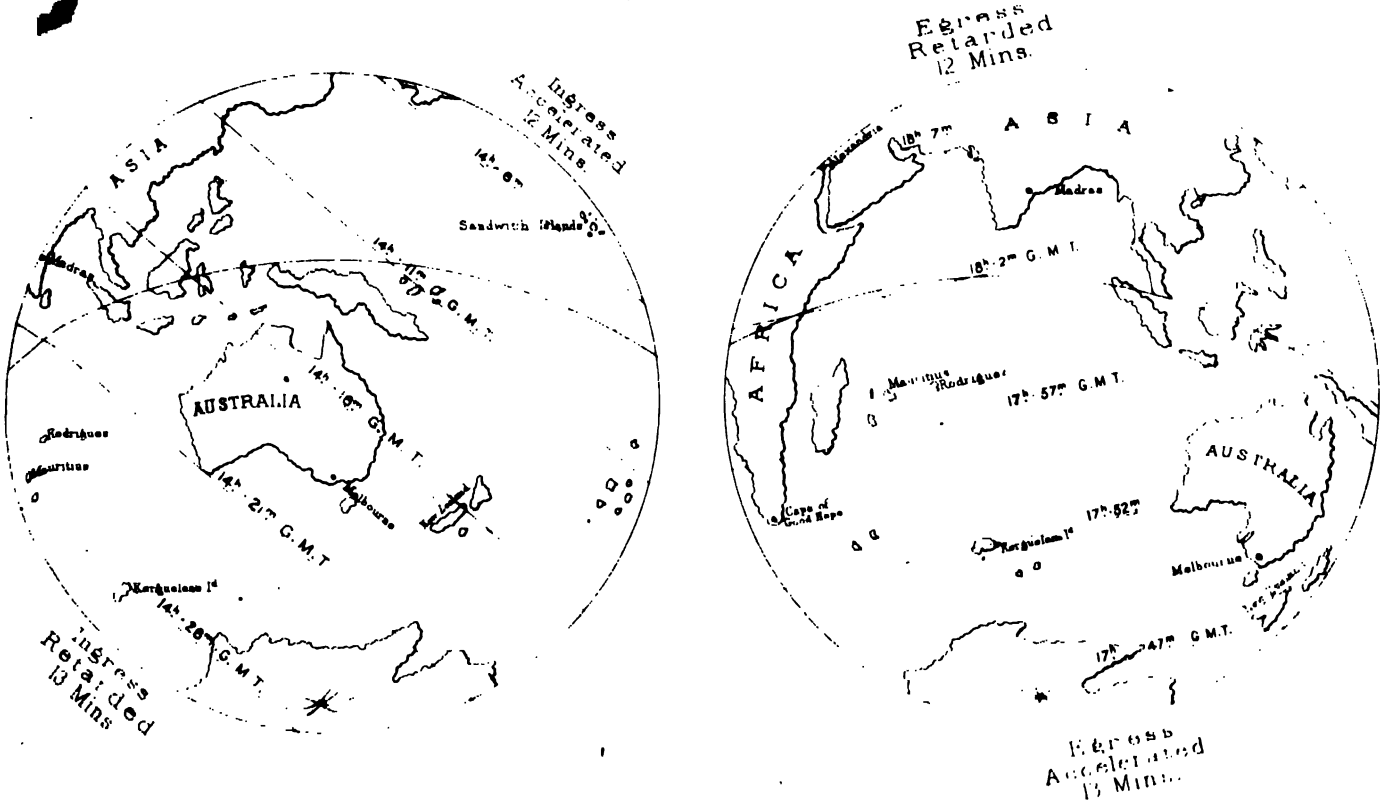
It is found that if a sensitized plate be over-exposed, the image of the Sun is enlarged considerably. This is due to photographic irradiation. It has been found,† by Lord Lindsay and Mr. A. C. Ranyard, to be chiefly due to reflections of light from the back of the plate. It can be almost entirely avoided by wetting the back of the plate and placing black paper against it. Still there will probably be a little enlargement of the Sun's diameter. This will not affect relative measurements of the distances of the centres of the Sun and Venus; but it will render it extremely difficult to determine the unit of measurement. There are two ways of applying the photographic method. The first is the same as the heliometric method. For this purpose, it is necessary to have one station in the north and another in the south. By the other method, we do not determine the least distance between the centres of the Sun and planet; but we determine the actual position of the planet at each observation. In other words, we determine the distance of Venus' centre from the Sun's centre, and also the angular distance measured from the north point of the Sun. To do this we must have, in the focus of the photoheliograph, a fine thread to indicate the direction of the meridian in the photograph.

The value of the different methods has been well discussed by De la Rue,‡ Tennant,§ and Proctor.‖ The method which takes into account the actual position of the planet on the Sun is the

* *Astronomische Nachrichten*, 1872, lxxix., 161.
† *Monthly Notices of the R.A.S.*, 1872, p. 313.
‡ *Monthly Notices of the R.A.S.*, xxix., 48 and 282.
§ *Ibid.*, 280. ‖ *Ibid.*, xxx., 62.

Proceedings of the Philosophical Society of Glasgow, 1872-73.

TRANSIT OF VENUS IN 1874.



Illuminated Hemispheres at Ingress and at Egress

more accurate, but it requires that the fiducial lines, or lines of reference, shall be exactly represented in the photographs. Mr. De la Rue says that this can be done to within one minute of space.

There are, then, at least four different methods for determining the Sun's parallax by means of a transit of Venus. These are the method of durations, the method of absolute time determinations, the photographic, and the heliometric methods. Which, then, are we to employ, and to what stations are we to send our expeditions?

Halley's method can best be understood by reference to the maps of that part of the world that will be illuminated at the time of ingress and the time of egress of the planet (Plate II.)

To fix our ideas, we may notice that places may be found at different parts of the Earth's surface, where the difference in time of Venus' contact with the Sun, amounts to about 25 minutes at egress or ingress. In the interval of four hours between the ingress and the egress, the Earth will have turned considerably round on its axis, as shewn by the maps. In this manner many places, where the ingress shall have been seen particularly early, will, after four hours, be so situated that the egress will be seen particularly late. In this manner the whole duration of the transit, as seen from some northern station, will be considerably lengthened. So also southern stations may be found so situated that the duration will be considerably decreased. For example, at Nertchinsk, the duration is $15\frac{1}{2}$ minutes greater than at the Earth's centre. But at Kerguelen's Land it is $16\frac{1}{2}$ less. Hence the difference in the durations at Nertchinsk and at Kerguelen's Land amount to 32 minutes. When we remember that the parallax is proportional to this difference in duration, you will see that great accuracy ought to be thus attainable. One second is only the $\frac{1}{1920}$ part of the whole difference in durations, so that each second of error, in this observed difference of durations, introduces an error of $\frac{1}{1920}$ of the whole value of the solar parallax, *i.e.*, about 0″·0046, or an error of 47,000 miles in the distance of the sun.

Now, is it an easy thing to measure the time of contact exactly? Past experience tells us that not only is it difficult to measure exactly the time of contact, but it is equally difficult to say whether or no it was the time of contact that you were observing. This kind of observation depends upon four of these observations of contact, each one of which is liable to some uncertainty. The total error may thus become large. But a consideration more important than this, is, that it is necessary to have a clear sky *down to near*

*the horizon** at the times both of ingress and egress, and this at both of the stations chosen for the observation. How does the second method contrast with this?

Let us look once more at the map. We may choose at ingress any places where the transit takes place much earlier and much later than if it were seen from the Earth's centre. For example at Kerguelen's Land, the transit takes place 12 minutes later than if it were seen from the Earth's centre, while in the Sandwich Islands it it takes place 11 minutes earlier, the total difference being 23 minutes. The difference between Rodriguez Island and the Sandwich Islands is 21 minutes. So, at egress, the difference between Alexandria and Auckland Island is 20 minutes, and between Alexandria and Kerguelen's Land is 15 minutes. These are all large quantities, so that a small error in the absolute time determination will make but little difference in the deduced parallax.† The chief difficulty here is to determine the *absolute* time at which the contact does take place. It is easy enough to find the *local* time, but not so the Greenwich time. It is easy to set our clocks to 12, at the moment the Sun is in the South, whether we be at Greenwich or Seringapatam. But a person at Seringapatam would find it rather difficult to set his clock at 12, when the Sun is in the south of Greenwich. The difficulty consists in finding the longitude.

But then there are great advantages in this method. There are only two observations of contact required in place of four. If the error of determining the longitude be less than the error of determining the moment of contact of Venus with the Sun—and there is every reason to think it is—then this method tends to be better than the other. Moreover, the observation of contact once made cannot be repeated. But the place remains, and its longitude may be redetermined. Of equal importance is the consideration that we are not nearly so dependent upon the weather when only two observations have to be made, as when there are four; so that such places as Possession Island, where we cannot be sure of the weather, are by no means so favourable as would appear at first sight.

Acting upon these considerations, nearly all the nations that are

* The sun will evidently be low at places best suited for observation. Atmospheric dispersion will come into play. To avoid this source of bad definition, Sir George Airy has devised a most ingenious eye-piece, in which any required amount of dispersion can be conveniently corrected by a prismatic arrangement. See *Monthly Notices*, xxx., 58. Mr. Simms has helped much in perfecting this important eye-piece.

† An error of 1 second will produce an error of about $\frac{1}{1800}$ of the whole parallax, or 0″·0074, and larger errors will act proportionally.

taking up the subject are following the lead of the Astronomer-Royal, Sir George Airy, in choosing their stations with reference to De l'Isle's method, although of course the other will be employed where convenient.

Before we go on to discuss the photographic method and the heliometric method, it will be well to take note what the peculiar advantage is of the observations of contact. Apart from the general circumstances of the transit of Venus, which is advantageous on account of its being nearer to us than any other planet ever is, there is this point. We are able to measure its position, as seen from different points of the Earth's surface, with the utmost precision. In the case of an opposition of Mars we have not this advantage. We can measure the position of the planet from each of the two stations with difficulty to 0″·5. But Venus takes 8 seconds to move over this distance of 0″·5. So that if the black drop be formed so instantaneously, as to cause a doubt of only one second, then the position of the planet, with respect to the Sun, is known to within $\frac{1}{8}$ part of half a second of space, *i.e.*, to 0″·06. We have here the most powerful means of measuring an angle that has been employed in astronomy.*

As to the photographic method, it is clear that the greatest effect of parallax for any two stations occurs when the line joining the two stations is as nearly as possible perpendicular to the line joining Venus with the Earth. In other words, to utilize any two stations to most advantage, observations must be made at times pre-arranged so as to satisfy this condition with respect to other stations.

This will give the greatest displacement possible for the two places. But here a point of importance arises.† The error in determining the distance between the centres of the Sun and Venus is small, but the error of determining a fiducial line—for example, the North point of the Sun—with which to compare observations at two stations, may be greater. Hence it may be advisable to pay chief attention, as Mr. Proctor has advised, to the least distance of the centres of Venus and the Sun. For this purpose it is best to choose for a pair of stations that time when the centres of Venus and the Sun are in the same plane as the two stations. In that case the difference of positions will lie wholly along a radius of the Sun. The probable error of a single measurement

* Exception should perhaps be made in favour of determining the R A of a star near the pole.

† This was pointed out by Mr. R. A. Proctor. *Monthly Notices, R.A.S.*

is 0″·1, and this would cause an error of 0″·0185 in the deduced paralax.*

As to the heliometric method, the choice of stations is the same as for photography by the second method. The principle is the same. But the measurements have a probable error three times as great, and the time cannot be determined with the same accuracy. Nor can the measurements be repeated as in photography.

As to the preparations that have been made by different countries it is impossible to say much. Few of them know well even now what they are going to do, and where they are going to do it. Britain forms a prominent exception to the rule.

The French are even now talking of an exploring party to go and hunt for suitable stations. They are to have five stations, and those that have been mentioned are Bourbon, Sidney, Pekin, and Yokohama. The nation has contributed 300,000 francs to the project, and the work has proceeded under a Committee.

The Germans have spent much trouble in experimenting on photographic distortion, and the artificial production of the black drop. Their probable stations are Chefoo, Auckland Island, Mac-Donald Island, and probably others.

They will employ all the four methods: eye observations of contact, heliometers, photographs of the distances of centres, and the position angles. Auxiliary stations will be sent to the Mauritius for heliometric and eye observations, and to Persia for the photographic method. There are to be 4 heliometers, by Frauenhofer, 3 ins. aperture, 3½ ft. focus; 4 equatoreally mounted telescopes, by Frauenhofer, 4½ ins. aperture, 6 ft. focus; 2 photoheliographs, by Steinheil, 5½ ins. aperture, and two more with quadruple object-glasses of 4 ins. aperture. Besides these, instruments are required for determining the local time and the longitude. For this purpose transit instruments, with diagonal telescopes 2⅔ ins. aperture, will be supplied, and altazimuths with divided circles 12 to 14 ins. diameter.

The Russians are going to devote themselves to those observations for which Siberia is eminently favourable, viz., the retarded egress and accelerated ingress. They will have about two dozen stations on the Coast of the Pacific, and from the East of Siberia up to the Caspian Sea. Each of the 24 stations is to have one instrument. There are three 4-inch heliometers, three photoheliographs, four 6-inch equatoreals, and four 4-inch ones, besides other instruments. The longitudes are afterwards to be determined at the successful stations by means of a line of telegraph to extend through

* De la Rue, British Association, 1872.

Siberia to Nicolajevsk. This promises to be a most valuable expedition when taken in combination with those of other nations. Great attention is being paid to the chance of fine weather.

The Americans have a grant of 150,000 dollars, and the work has proceeded under the management of a Committee. They have paid great attention to photography under the guidance of Mr. Rutherford. They will measure both angles of position and distances from the centre. The probable error at any station will be less than the $\frac{1}{10000}$ of the distance measured. Further, they propose to use a lens of 40 feet focal length to free the image from optical distortion, and this lens will remain fixed while the Sun's image will be reflected by means of a heliostat. A most careful report has been drawn up of the relative values of different stations.

From Britain two expeditions have been organized, the one under the superintendance of Sir George Airy, the Astronomer-Royal, which is the work of the nation. The other, a private one, perhaps the most completely-equipped private expedition that has ever been undertaken in the service of astronomy. Lord Lindsay has determined to establish himself at the Mauritius, equipped with the most perfect instruments obtainable for all the different modes of observation. In common with the French, he proposes to observe not only the internal but also the external contact. This is a matter of little difficulty, since, by the help of the spectroscope, we can see the chromosphere surrounding the sun, and Venus will be visible when advancing through it. The observations will be made by Lord Lindsay and Mr. Gill with proper assistance.

For reflecting the sun's light through a lens of 40 feet focus, he employs a siderostat like that made by Foucault for the Paris Observatory. He will not measure angles of position by photography.

It is to be hoped that some station, with which the comparison is to be made, will be provided with similar instruments.

Lord Lindsay and Mr. Gill lay considerable value on the heliometric method, and have discussed its capabilities with great lucidity.

Few of the instruments are as yet completed.

The English Expedition may be said to be almost ready. £15,000 has been voted by the country for the undertaking, and the whole management of the expedition has been given to the Astronomer-Royal; but Mr. De la Rue has presided over the photographical department.

The greatest pains has been taken to secure good instruments. Moreover, all the instruments are as nearly alike as possible, a most important point when irradiation is taken into account. There are

five stations, viz., the Sandwich Islands, Kerguelen's Island or the Island of Desolation, New Zealand, Rodriguez, and Alexandria*. Each station will be provided with a 6-inch equatoreal, and a 4-inch one. There will also be a transit instrument for determining local time, and an altazimuth for making observations of the moon by means of which the longitude may be determined. There will also be a photoheliograph for each, several clocks, and other instruments.

Now, nearly all these instruments are ready, and portable observatories have been prepared to receive them. I have a photograph by means of which you may see a group of these huts as they were arranged in the south ground of the Observatory of Greenwich last summer. The large building is for an equatoreal, the small hexagonal building contains an altazimuth, while to the left of the figure is seen a hut for containing the transit instrument. In the foreground are three or four transit-instrument huts, with the heavy stone piers, all packed and ready for transport.

I have another photograph shewing the transit instruments to be employed.

But not only are the instruments all ready, but observers have also been collected, and some are already fitted for all the observations. Further, the printed forms into which all the observations must be entered are prepared; so that everything is ready and there is a year to spare in which to think calmly over the matter to see if anything more can be done.

Sir George Airy has decided in favour of De l'Isle's method, because the longitudes of places can be determined, he hopes to one second of time. Still there is one station, Kerguelen's Island, that is as well suited as any other in the south for employing the method of durations. The time of ingress is retarded by 11 minutes, and the time of egress is accelerated by 6 minutes; hence the duration as seen from Kerguelen's Island will be about 17 minutes shorter than if seen from the earth's centre. Combining this with some Russian station when the duration is 18 minutes, we have a difference of 35 minutes to deduce the Solar Parallax form. Possession Island is another with exactly the same advantage as to time that Kerguelen's Island has. It has been proposed to remove one of our stations to this rock. But remember that we require all our stations to complete De l'Isle's method. Shall we then destroy the completeness of the one

* There will also be a station in Northern India, and, of course, the observations at the Cape of Good Hope, Madras, and Melbourne will be utilized.

method to go in for a half-and-half measure? Shall we risk the loss of one method to gain the chance of observing in company with, and in dependence upon, the Russian party, whose instruments, being different from ours, are unfitted for comparative observations with ours? No. It is decidedly more wise to abide by the present arrangements, which have been accomplished by a generalship quite unparalleled in the annals of science, that has utilized to the utmost the disposable funds, and of which we, as a nation, ought to be proud.

If the Government propose to supply funds for such an antarctic expedition, by all means let it be done; the gain would not be for astronomy alone, but for every branch of science.*

If any hints, from one who has given considerable attention to the subject, were likely to be thought of use, I would mention a few points:—1. That where practicable, *e.g.*, in the Sandwich Islands, where there are mountains, and where the weather can, I believe, be depended upon, the observatories should be carried to as high a point as possible; 2. That the state of the edge of the Sun, as to protuberances, should be carefully observed at different observatories on the day of the transit; 3. That the dark glasses employed should be as definite in colour as possible.

There cannot be the slightest doubt that phenomena of nature derive their sublimity from the thoughts which they raise up in our minds. A peal of thunder is sublime. The rattling of a cart is not so. Yet the one can at times be with difficulty distinguished from the other. The meteor shower of November 27, 1872, was a *beautiful* spectacle to every one who witnessed it; but it was *sublime* to those who were aware of its having been predicted, and who knew that each meteor, as it flashed through the sky, was proclaiming, in unmistakable language, the solution of a riddle that had puzzled the wisest—what had become of Biela's comet? The Sun would be none the prettier for having its brilliancy marred by a round black spot of uniform darkness. But to those who know all that this spectacle unfolds to us, the phenomenon becomes one of the utmost grandeur; for we then see Nature in the act of making one of the signs which go to form that complete language in which she is accustomed to speak to the philosopher and expound the problems of the universe.

Let us then hope that those who go out to observe this rare spectacle may be more favoured than was Le Gentil. He went

* Provided the explorers lived to tell the tale, concerning which it would be foolish to be hopeful.

out in 1761, but being wrecked, he arrived at his outlandish station too late for the transit. But, being an enthusiastic astronomer, he set up his instruments, and waited patiently for the transit of 1769. When the long expected day at length arrived—the sun was obscured by clouds.

XIX.—*Examination and Defence of the Principles Enunciated by Mr. Lowe in his letter to the Chamber of Commerce, Glasgow, of the 20th December, 1872.* By Mr. William Miller, *Banker.*

Read before the Sanitary and Social Economy Section of the Society, March 7, 1873.

Prayer of the Memorial from the Chamber of Commerce to Mr. Lowe, to which his Letter was a Reply.

Your Memorialists, therefore, earnestly urge that the provisions of the Act of 1845, relating to the making or issuing of Bank-notes *should be repealed*, and *the right* to issue Bank-notes payable on demand *freed from legislative interference.*

Mr. Lowe's Letter to the Chamber of Commerce.

"11 Downing Street, Whitehall,
"*20th Dec., 1872.*

"Gentlemen,—I am sorry to find from your memorial that, in your opinion, the Act of 1845, with regard to the Bank issues in Scotland, has created a monoply and increased the profits of banking at the expense of the mercantile community. But I cannot agree with you that the remedy is to be sought in the repeal of the Act of 1845, and in freeing the issue of notes payable on demand from legislative interference. *Formerly the issue of Promissory notes payable on demand was regarded as part of the business of a banker. Now it is, I think, generally recognized that the issue of such notes is the creation of money, and that the creation of money is the business of the State, not of any trading association. Hence it follows that the issue of such notes is rather an anomaly which we may tolerate, than a right which we ought to extend.*

"A mixed currency, composed partly of the precious metals and part of paper, cannot be in a sound condition unless it complies with the following three conditions:

I. "The paper must be convertible into gold on demand.

II. "Sufficient security must be held for the issues to secure the payment of the notes.

III. "The mixed currency must be at all times of exactly the same amount, and consequently of the same value as a purely metallic currency would be.

"The plan which you propose does not fulfil the third condition, and appears to me, therefore, inadmissible. It may, perhaps, be worth considering whether some relief might not be afforded, and some facility given for the establishment of future banks in Scotland, if Bank of England notes were made legal tender north of the Tweed. I make this suggestion because I observe that English banks are also limited in their issues by the Bank Act of 1844, and that this limitation does not prevent the establishment of banks without any privilege of issue, but carrying on their business with the ordinary currency of the country.

"I am, Gentlemen,

"Your obedient Servant,

"ROBERT LOWE.

"TO THE GLASGOW CHAMBER OF COMMERCE AND MANUFACTURES."

Let me first ask your attention to the exact terms of the Memorial. The Chamber of Commerce pray "That the provisions of the Act of 1845, relating to the making or issuing of Bank-notes, *be repealed.*" They speak of the power to issue Bank-notes as a "*right,*" and they ask that *this right* be "freed from *all legislative interference.*"

In reply to this Mr. Lowe first enunciates three principles, which may be put in the form of a syllogism, thus :—

1. The issue of Bank-notes is the creation of money.

2. The creation of money is the business of the State; and the conclusion he draws is—

3. Therefore the issue of Bank-notes *ought to be* subject to legislative interference.

Now, in order to understand exactly what is here asserted, we must make sure that we know the precise meaning of the terms employed. What then does Mr. Lowe mean by "money?" There are two senses in which this word is commonly employed.

1. It is sometimes used to mean "Loanable Capital;" as, when we speak of "The Money Market," "The Price of Money," &c.; or sometimes even capital of any kind, as when we say—"Such a one has plenty of money"—"he is making money rapidly."

2. But the primary and more correct meaning of the word is "Currency;" and this is the only sense in which Mr. Lowe uses it in his letter. His argument therefore is—

1. The issue of Bank-notes is the creation of currency.
2. The creation of currency is the business of the State.
3. Therefore the issues of Bank-notes ought to be subject to legislative regulation and control.

To be more specific, "Money," as used here by Mr. Lowe, includes all those instruments of ordinary and universal exchange in a country, whether consisting of coins of the realm, or of Bank-notes *in circulation*, which are constantly recognized, and accepted, and used by the community generally, in the payment of debts, the purchase of commodities, and the final settlement of transactions.

It is admitted that there are other instruments of exchange in use among commercial men, such as bills and cheques, which perform some of the functions of currency; but these differ from the others in many important particulars, such as,—that they are not in universal imperial use—that they do not circulate freely from hand to hand as the others do—that they could not be, from the very nature of them, accepted by all and sundry as of equivalent value with the Standard of Value—that their use ceases, generally speaking, with the performance of only one transaction—that they are employed only in settlement of the larger and more strictly mercantile transactions—and that they are almost wholly unknown in the much more numerous transactions which are constantly taking place in the humbler ranks of life, in the payment of wages, in the purchase of provisions, and in the innumerable smaller exchanges which make up the routine of every-day existence. Bank-notes, on the other hand, when issued by companies of undoubted and universally recognized credit, perform *all* the functions of coin of the realm; enter into the discharge of every transaction, however minute; are everywhere, and by every one, accepted as of equivalent value with the gold which they represent; and are consequently universally received as complete and final settlement of all obligations, even of those very documents—bills and cheques, viz.—with which they have so often, but so erroneously, been bracketed.

It is no argument to say, as is so often done, that there can be no specific or essential difference between them and those other documents, for both are *in their nature* mere promises to pay, and nothing more. Neither is it any argument to urge that they cannot be regarded as money of the realm, because they have no intrinsic value as coins have. Both of these statements are, of course, admitted to be true; but I cannot, for the life of me, see that they have any bearing whatever upon the question at issue—the only considerations which determine to what category Bank-notes belong, being, as I

think, that they discharge all the functions of money, and are universally regarded and treated as such.

The editor of the *Financier*, in criticising Mr. Lowe's letter, makes the following startling statement:—"There is a confusion of ideas, or, what is worse, of facts, in Mr. Lowe's remarks above quoted. In the opening clause of this last sentence he uses the word 'money' in one sense; and, in the subsequent clause of the same sentence, he uses it in another, and substantially different sense;" and then the editor goes on to argue that this accounts for all Mr. Lowe's confusion of ideas on the subject. Now, I would respectfully suggest to the editor whether it may not be within the bounds of possibility that he, not Mr. Lowe, has made a mistake. There can be no doubt, I think, that Mr. Lowe, who generally knows pretty well what he is talking about, uses the word "money" in both places as interchangeable with "currency," and that he looks upon Bank-notes, in circulation, as currency, quite as much deserving this title as the current coins of the realm, and that consequently they, equally with these coins, demand the attention of the Government.

I am quite aware, of course, that there are many men of eminence who hold, or used to hold, an opposite view to this; but I think Mr. Lowe is correct in believing that statesmen, and scientific men too, are now generally of opinion that the issue of Bank-notes, if not, in the strictest sense, the creation of "money"—*i.e.*, money of the realm—is yet so entirely equivalent to this, in every respect, as to be practically identical with it. At least, this I am persuaded is the view of Mr. Lowe, the criticism of the *Financier* notwithstanding. It is the view, we know, of Mr. Gladstone; and, undoubtedly, it was the view entertained by the late Sir Robert Peel.*

"The creation of money is the business of the State." Money, I have said, is the universally recognized medium of exchange in all the smaller transactions of commerce, and of everyday life. It is something with which every man, however poor or however unintelligent he may be, has to do. It is therefore of the utmost importance, rather, let me say, it is absolutely necessary, for the conducting of these transactions with perfect satisfaction to all parties engaged in them, that there should be no doubt whatever in the minds of any one as to the absolute security, and exact value of this medium of exchange, in whatever shape it may appear. As regards *metallic* currency, or coins which circulate from hand to hand, and form one most important portion of this universal instrument of exchange, there has never been,

* See Appendix—Opinion of Sir. R. Peel.

so far as I know, any hesitation on the part of intelligent men in recognizing the right to issue them as the prerogative of the Government of the country in which they circulate. "Shew me a penny," said one who knew well what was due to the powers that be, when questioned as to the prerogatives of an existing but foreign Government. "Whose image and superscription is this?" "They say unto Him, Cæsar's." "Then," said He, "render unto Cæsar the things that are Cæsar's." But it is not needful that I should argue this matter before an audience who know that it has been the universal practice for all civilized governments to keep in their own hands the creation of the metallic currency of their respective States. In a recent publication of great merit, I have been surprised to find that this State privilege of coining is made light of. The author says, "The duty performed at the mint is purely mechanical. The quality of the metal is proved, the weight of the coin is ascertained, and the Government stamp or certificate is affixed. There is no prerogative in the matter—there is not even any idea of profit. The coinage is manufactured at the mint, simply as a matter of expediency, with the view of securing to the public the highest assurance of the genuineness of the coins." Now, I confess, I am unable to perceive the force of this argument, or to understand why it should be considered to be an argument at all. It is true, of course, that the process by which this security to the public is given may be described as a purely mechanical one; but why this should of itself be supposed to prove that "there is no prerogative in the matter," I utterly fail to see. As well might we say, the signing of a State paper—say a Declaration of War—is purely mechanical, therefore it can form no part of the Royal Prerogative. The editor of the *Financier*, reduplicating on this same argument, takes grave objection to the term "creation," as used by Mr. Lowe. "The creation of money," he says, "is a large-sounding phrase; but what the State really does in this matter hardly corresponds with the magniloquence of the phrase. The State certainly does not provide or 'create' the material of the coinage—it simply mints the specie brought to it, converting the bullion into coins of a certain weight and fineness." Now, this appears to me to be the veriest trifling in the shape of argument. No one, surely, requires to be informed that the attribute of omnipotence does not belong to, and is not arrogated by, any government, or that Mr. Lowe had no such idea in his mind when he wrote of the "creation of money." What he evidently means by the phrase is just what the *Financier* so pompously proclaims to be the fact, viz., that the process of coining at the mint converts that which was formerly the raw material of bullion into the manufactured article, money or currency—

thus rendering that which was previously of no value, as a medium of general exchange, eminently serviceable in that capacity. The skilful mechanic who fabricates out of a bar of iron some ingenious machine, which shall be fitted for discharging functions altogether higher and different from those which could have been discharged by the shapeless block of metal, may be said, in popular parlance, to create the machine; and, if he be the inventor as well as the fabricator, and has accordingly obtained a patent for its manufacture, he may very properly claim the creation of this machine as his peculiar prerogative. And it will be no answer to this claim of his to say, "You did not create the matter of this machine—all you have done is purely mechanical."

The editor of the *Financier* further urges, in support of his objection—"Moreover, it sometimes happens that the State deputes the work of coining to private establishments, like those of Birmingham." But how can this help his argument? The State, he says, "*deputes*" the work to others. Does not this very expression of his prove that those others are merely the servants of the State, and only work to its dictation, just as much as if they were its salaried officials? The *Financier* does not surely mean to argue that the State cannot be said to do this work unless it has been performed by the cunning hands of Mr. Lowe or Mr. Gladstone; and he fails to see, I think, that, by the very argument which he is here using, he has unwittingly suggested an analogy between coining, and issuing notes, which goes in direct opposition to the views which he is seeking to establish; for, if the mechanical act of coining may be delegated to private establishments in Birmingham, and yet remain the prerogative of the State, why may not the privilege of issuing notes be delegated to public companies in Scotland, without the relinquishment of that right to create all currency which Mr. Lowe claims on behalf of the State?

Assuming, then, as I think I am entitled to do, that the right of furnishing a metallic currency belongs alone to the Government, and adding this further consideration, already disposed of, that the common paper currency of the country—*i.e.*, Bank-notes in circulation—performs exactly the same functions, and is practically, in every respect, equivalent to the metallic currency which it comes in place of, there cannot, I think, be any hesitation whatever in concluding that the creation of this currency also is, just as much as the other, the business of the State; and, although the Government may find it better, all things considered, to leave the practical working of this paper currency to the Banks, that they have nevertheless a perfect right to control

and regulate that working in any way, or to any extent, which they may consider needful in the interests of the community at large. "The Bank Act of 1844," said Mr. Gladstone, "has laid down a principle of great importance, and one which I hope will never disappear from our Statute Book, but will, on the contrary, acquire increased recognition as circumstances may permit—I mean this principle, that the whole business of issue is the business of the State, and that where that function is exercised by any body other than the State, it must be considered as exercised by delegation only."

The grounds of this doctrine, in addition to that of prerogative, appear to me to be these :—That the currency of any country, being called on to perform functions which affect the interests of the community at large, and in which the poorest member of that community has a direct and personal interest, ought to be rendered, in every respect, fully efficient for the performance of these functions; that it cannot be fully efficient for this purpose, unless all parts of it possess an equal value, and maintain an equal credit, at all times, under all circumstances, and at all places of the kingdom at large; and that the duty of seeing that this is the case can only be, and therefore ought to be, discharged by the Government of that country. In other words, the currency of the country is a national matter, performing national functions, and effecting national interests, and ought therefore to be either the creation of the State itself, or, at the very least, subject to national regulation and control.

In connection with this part of the subject, it is instructive to notice that many of those who are most zealous on behalf of what they call "Free Trade in Banking," by which, however, they really mean "Free Trade in Issues," are, at the same time, most inconsistent advocates of certain restrictions and regulations being necessary on the part of Government on this very right of free issue which they claim. Thus Mr. George Smith, the other day, while denouncing most vigorously the monopoly of issue enjoyed by the Scotch Banks, and pleading for all issues being freed from legislative interference, indicates his view to be "in favour of even the existing Banks having their issues based on consols or gold, the same as the Bank of England, leaving the question of present and future issues *to be dealt with by Government*." And the ingenious author already referred to, while declaring at one time that "State interference for the purpose of requiring the deposit of gold, or other specific securities, *is opposed to sound principles*," at another time admits that "it may be matter for consideration whether it would not be prudent to require the Banks *by law* to maintain in coin a certain definite and important proportion

of the amount of their issues"—that is to say, he thinks it may be prudent to do that which he believes to be opposed to sound principles! Both of these gentlemen thus unconsciously admitting that the issuing of Bank-notes is something altogether different from the ordinary operations of commerce, and that this privilege of issue, for which they contend, is one which, if unrestricted, may, by abuse, result in serious detriment to the general interests, and ought therefore, to some extent or other, to be controlled or restricted by Government. Now it is clear, I think, that, by admitting the necessity for any restriction or regulation whatever, they admit the principle for which I am contending, and that it must be left to the State to carry out that principle in practice, and to determine what and how great this restriction shall be. If, therefore, the State should declare that, on review of all the experience of the past, they are of opinion that this privilege cannot be exercised safely by public companies at all, and must therefore, for the future, not only be subject to regulation and control by the State, but be altogether in the hands of State officials, there can be no ground of *principle* on which these gentlemen, of whom I speak, can oppose even so extreme a measure as this would be. They can, of course, urge arguments, *founded on expediency*, against the details of any measure introduced by the State for the regulation of the currency, but they cannot oppose to it the plea that it would be the infringement of any popular right.

The idea of a single State Bank of Issue has been frequently denounced as opposed to all sound economic principles; but it is not one whit more so than the idea of a single State Post-Office Co., or a single State Telegraph Co., or a single State Railway Co.; and yet the first has long been an accomplished fact, the second has lately been successfully organized, and the third has been seriously discussed more than once of late; but I do not remember that any of our staunch Free-traders have found it necessary to oppose themselves to any of these schemes on the principles of their science—the one principle which has ruled in their discussion or adoption being that the greatest good of the greatest number, or, in other words, that national interests will, in those instances, be better promoted by national than by individual administration.

I trust I will not be understood to be arguing just now in favour of a single State Bank of Issue, as against the plurality of individual Banks of Issue which presently prevails in Scotland. I am quite prepared to admit, or rather to urge, that, as regards Scotland, taken by itself, the existing system of issue, somewhat modified perhaps as regards the nature of the security held, is as near perfection as

the circumstances will admit of. I am only seeking to establish the soundness of those principles enunciated by Mr. Lowe, on which, no doubt, it is alone possible to defend the proposal to establish a single State Bank of Issue, or even the present laws which regulate the currency of the country; and these principles I take to be, as follows:—

1. The issue of Bank-notes is the creation of money, *i.e.*, currency.

2. It is the business of the State to create currency.

3. Therefore it is the business of the State, on the ground alike of prerogative, and of a regard to the general good, to control and to regulate the issue of Bank-notes.

From this it will of course follow, that the Bank Acts of 1844-45, however defective in detail or injurious in practice they may sometimes be, are not opposed in theory to the doctrines of Free Trade; and this also will follow, that it is on these principles, enunciated by Mr. Lowe, that the exclusive privilege of issue enjoyed by the Scotch Banks can alone be defended. That it can thoroughly be defended in this way I have not the smallest doubt, for I have the utmost confidence in the truth of the principles.

I now proceed to the consideration of the other group of principles enunciated by Mr. Lowe. He says—"A mixed currency, composed partly of the precious metals and partly of paper, cannot be in a sound condition unless it complies with the three following conditions:—

"1. The paper must be convertible into gold on demand.

"2. Sufficient security must be held by the issuers to secure the payment of the notes.

"3. The mixed currency must be at all times exactly of the same amount, and consequently of the same value, as a purely metallic currency would be."

The first two of these need no remark, as their truth is universally admitted: it is with the third only we have to deal. And here, again, it is necessary that we first of all comprehend aright what is meant. The editor of the *Financier* says that the "obvious meaning of the proposition, and the only sense in which it is correct, is, that in a united currency of gold and notes, the notes shall at all times possess the same value as the coins which they represent." In other words, that a paper pound shall always be equal in value to a pound in gold. Now this is not its meaning, and the *Financier* knows very well that it is not; for he goes on subsequently to explain, though by no means correctly, what he thinks Mr. Lowe really does mean; and then having given his own caricature of the statement, he dismisses it with contempt, as "an antiquated doctrine now altogether abandoned by even the staunchest supporters of the Bank Acts." But instead of taking the

Financier's exaggerated statement of the doctrine, let us in fairness take it from the mouths of those who have most conspicuously expounded and defended it. What they really did mean, then, we may gather from the following. Col. Torrens, who was perhaps the most logical of them all, says—"The doctrine of Mr. Ricardo is, not that the value of a convertible Bank-note can be lowered in relation to the coin for which it can be immediately exchanged, but that the value *both of coin and note* may be lowered *in relation to foreign currencies.*" And again —"It will not be disputed that under the present mixed system the Bank possesses the power of meeting a demand (for currency) by creating more notes, and of their maintaining *for a time* a more extended circulation than that which would exist under a purely metallic currency. This, I repeat, is all that the advocates of the metallic principle have ever contended for. They have never for a moment imagined that Banks of Issue possess the power of so forcing out their notes as to give to the currency, while convertible into gold upon demand, *a permanent* expansion greater than that at which a purely metallic currency could be maintained." And then he goes on to argue that means should be taken to keep in check all such undue expansions of the currency, which, if not counteracted in time, may lead to depreciation of the entire currency, and to consequent export of gold; and especially to make sure that, when gold has been exported, its place shall not be supplied by a further issue of notes. The doctrine of these gentlemen, then, we see, is this—they believe that it *is* possible, under a mixed currency, to create a *temporary* over-issue of notes; that this over-issue has the effect of depreciating the entire currency in comparison with that of foreign currencies; that this depreciation leads inevitably to an export of gold; and that the vacuum in the currency, thus created, ought not to be filled up by notes.

Many of those who object to this doctrine have entirely misunderstood, and therefore unintentionally, of course, misrepresented it, by failing to notice, or at least to take count of, the meaning of the word "*circulation*," as understood by the promoters of the Bank Acts. Their use of the word, I grant, is inexact and misleading; but, if we are to arrive at a correct comprehension of their proposition, as stated by themselves, we must first of all, in fairness, understand exactly what meaning they attach to the terms which they employ in stating it. Now, Col. Torrens informs us that by the term "*circulation*" he means "Coin and Bank-notes, whether held for use in the drawers of individuals, in the cash-boxes of trading concerns, or *in the coffers of banks.* It is in this sense," he adds, "that the term has been employed by the promoters and advocates of the Act of 1844." And

Mr. Lloyd, when asked by the Committee of Inquiry on Banks of Issue, "What do you consider to constitute the circulation?" replied, "Notes in the hands of the public, and in the *Bank till.*" Taking along with us these explanations, we see that the argument maintained by these gentlemen, and now again enunciated by Mr. Lowe, may be stated thus:—As an export of gold would, under a purely metallic currency, reduce the amount of gold *available for circulation*—*i.e.*, whether in the hands of the public, or in the coffers of the bank; and as, under a mixed currency, the amount of notes and gold *available for circulation* should not exceed that which it would have been had the currency been purely metallic, it is therefore necessary when, under a mixed currency, an export of gold has taken place, that the entire amount of gold and notes available for circulation should be reduced by the amount of gold exported.

And now, in proceeding to the discussion of this proposition, as thus stated and explained, it is necessary, first of all, that we enquire somewhat into the practical working of a paper currency, and try to discover whether it exhibits any tendencies to differ, in its operation, from that of a purely metallic currency. In other words, whether a mixed gold and paper currency has a greater tendency to over-issue than a purely metallic currency would have.

Bank-notes are not capital; "they are merely promises to pay" capital. Neither can the issuing of them be said to be the creation of capital, but they are exceedingly well adapted to convey or transfer it. They have been compared to parcel-vans, which go out from the Bank counter, distributing capital all over the town and country to the customers of the Bank, and returning, after a time, thoroughly emptied of their contents. But this is not an adequate, or even an altogether correct illustration, for it is necessary to keep in mind that, while they undoubtedly distribute capital, of most excellent purchasing power, to the Banks' constituents, and, through them, to the public generally, delivery of the capital so distributed, strange to say, is not demanded of the Banks until after return of the vehicles which have conveyed it. There is therefore a time varying, according to circumstances, from a few days to several weeks, but averaging upon the whole, say ten days or a fortnight, during which the Bank's customers pay for the use of the capital distributed, without the Bank being called upon for delivery. And it is here that the great temptation, under a free and unrestricted system, such as prevailed prior to 1845, lies to over-issue of notes; for it is clear that the more of his notes a banker can place in the hands of the general public, and the longer he can keep them floating there, so much the greater will his profit be.

Now, I am ready to admit all that is urged on behalf of the efficacy of those counter-checks, which naturally exist in opposition to this temptation and tendency to over-issue—such as the prudence of the banker issuing, the law which enacts that the notes must be paid in gold on demand, and the admirable system of exchanges which prevails in Scotland among the various Banks, by means of which, in Scotland at least, the danger of a free issue would undoubtedly be reduced to a minimum. Still, I am bold to maintain that no such temptation *ought to be* presented to any banker, however prudent he may be; that in times past it did, over and over again, prevail so as to overcome all these counter-checks; and that it would most assuredly do so again were unlimited and unrestricted freedom of issue permitted as before. This last is, of course, only matter of opinion, and may be denied by those who contend for free trade in issues; but my first and second statements cannot be denied by any one who is old enough to remember the practical working of this freedom of issue in times gone by. Prudent bankers there were in plenty, I know; but there also did exist, as there always will exist, so long as man is fallible, those who were exceedingly imprudent, and who were tempted by this, among other influences, to bring to ruin, not only the establishments over which they presided, but also thousands of unfortunate victims who were innocently dragged down along with them. Let me give but one instance. I began my banking career in the north of Ireland in the year 1837, and was therefore in time to witness the deplorable results produced in that country by the failure of the Agricultural and Commercial Bank, which had spread itself all over the province, and had entered upon a short-lived but mad-like competition with the other and older Banks in the circulation of its notes, and, by *means of this*, in a profuse advancing, to unsafe customers, of the hard-earned savings intrusted to its care. The failure of a Bank is no common calamity. It is not a few moneyed men, or speculative merchants, who alone suffer, as is the case in ordinary bankruptcies; but here it was the thousands of poor labourers or artizans, who had been tempted, by the flaming professions of a few reckless speculators, to become shareholders or depositors. It is all very well to point, as many do, with justifiable pride, to the ultimate liquidation of all their obligations *to the public* by the shareholders of, let me say, the Western Bank; but those gentlemen never seem to take into consideration the countless and unknown miseries suffered by so many widows, and orphans, and other small annuitants during the progress of the liquidation. And surely, on this ground alone it is the business of a paternal Government to interpose all possible constitutional checks to the evil tendencies of a

system which, in other respects, is productive of so much substantial benefit.

But this brings me back to my more legitimate argument, which was to shew that, in free trade in paper currency, there *is* danger of over-issue. The argument used in reply to such a statment is—"such a thing is impossible, because, under the present perfect system of checks, the notes issued beyond the legitimate requirements of trade would at once be returned upon the issuer." This is plausible, but, I venture to say, superficial. It does not go down to the root of the matter. I have said that the issuing of Bank-notes conveys capital from the Bank to the customer, and that there exists in connection with the issue of a paper currency a strong temptation to the issuer, to convey away *by means of it* more of his capital than perfect prudence would dictate, or than the *legitimate* demands of the commerce of the country would warrant.

In further illustration of this, I might go on to refer to well-known facts and to the opinions of some of our most distinguished economists;* but your time will not permit of this, and I must therefore be content by reasserting my conviction, arrived at by a pretty close study of the history of banking during the last thirty-six years, that Bank-notes, if wholly unrestricted, could be, and most inevitably would be, over-issued—that is to say, they could be issued so as to create, or at least to pander to, an unhealthy demand for capital—and so as to induce disorder in the body commercial, just as it is possible, your chairman will tell you, to create, by means of stimulants, an unhealthy appetite for food, so as to produce disease, or it may be death, in the body natural.

Now it is admitted that the same over-issue of currency and, through that, of capital, might be made in a time of commercial excitement, although the currency were purely metallic; but, in that case, the element of profit, by the circulation of the currency in use would, of course, be entirely wanting, and bankers would be left wholly free to be guided by those prudential considerations which should alone determine the giving or withholding of their advances.

What is wanted, then, is, that the temptation to over-issue, which is the natural result of a system of free-issue, should be, in some way or other, counteracted, and that means should be taken to ensure that the action of a mixed currency should exactly correspond with that of one purely metallic.

But in order to ascertain how this may best be accomplished, it is

* See Appendix—Mr. Tooke and Mr. Fullarton as to "Free Trade in Issues."

necessary that we enquire somewhat more particularly into the manner in which an over-issue of notes operates in disturbing and rendering unhealthy the commerce of the country. The first effect of an over-issue of notes would undoubtedly be to depreciate the currency. "If an addition be made," says M. Ricardo, "to a currency partly of gold and partly of paper by an increase of paper currency, the value of the whole currency would be diminished, or, in other words, the price of commodities would rise, estimated either in gold or paper currency. The same commodity would purchase, after the increase of paper, a greater number of ounces of gold coin, because it would exchange for a greater quantity of money." And this leads inevitably to a depreciation of the currency in relation to foreign currencies, and consequently to an exportation of gold. Mr. M'Culloch, in his edition of Smith's *Wealth of Nations*, says, "The effect of inordinate issues of notes, immediately convertible into gold, is not, therefore, to cause any discrepancy between the value of paper and the value of gold in the home market, but to increase the amount of the currency, and by rendering it redundant or depreciated, *as compared with that of other countries*, to depress the exchange; and thus, inasmuch as notes do not circulate abroad, to cause a drain upon the Bank, and the exportation of coin." And even Mr. Tooke himself seems to admit the validity of the same argument, when he says, "By excess, I mean such an enlargement of the circulation, through the medium of paper, as cannot be maintained at a level with the value of gold in the rest of the world, to which we must eventually *conform*."

But it may be asked, "Why should the exportation of gold be a natural consequence of the depreciation of the currency?" The answer, as given by Col. Torrens, is, "Because its value in relation to exportable commodities has fallen; in other words, only because the price of commodities have risen. Before the quantity of money was increased by the issue of Bank-notes, our imports were paid for by the export of commodities; because, at the then existing low ranges of prices, commodities were the cheapest instruments by which foreign payments could be effected. After the quantity of money has been increased by the issue of Bank-notes, our imports are paid for by the exportation of gold; because, at the now existing higher range of prices, gold is the cheapest instrument by which foreign payments can be effected."

The argument of these gentlemen then is this. The result of an over-issue of notes is to depreciate the currency in relation to foreign currencies, and the effect of this again is to lead inevitably to an

export of gold: and hence they conclude that the means which should be taken to correct this state of matters, and to bring back the gold, must be one which tends directly towards an appreciation of the currency, by a restoration of the issues to that normal state from which they are supposed to have departed; and this, looking at the operation of the Bank Act, we find to be a raising of the rate of interest.

This doctrine is contemptuously dismissed by the editor of the *Financier* as antiquated and long ago exploded. The editor of the *Economist* treats it somewhat more respectfully, for he says, "If a country, which uses a credit currency founded on gold and silver, *fails to diminish that currency* when gold and silver diminish, its range of prices will be different from that which, under a purely metallic currency, would prevail, its exports will be less, its imports will be more, and the balance of its trade will be unredressed." "Nothing is so important, therefore, as that the currency should be made so *to expand and contract*, as to keep the prices the same as they would have been had the currency been purely metallic." And the means of effecting this he finds to be exactly the same as that aimed at by the promoters of the Bank Acts; for he continues—"The best method—the sole method—to contract the entire currency in this or any other country is to *raise the rate of interest*."

And Mr. Wilson, the former editor of that paper, says, in his "Capital, Currency, and Banking," when referring to the same subject—"We are ready to admit most fully that whatever is the action on a purely metallic currency in the case of an efflux or influx of bullion, arising from an adverse or favourable exchange, the same precisely ought to be found to exist on a mixed currency of coin and convertible paper."

But there is another, and, as I think, a more palpable effect of an over-issue of notes which leads precisely to the same result in the exportation of gold, and may therefore, I suppose, be looked upon as a concurrent phenomenon with that of depreciation of the currency. I mean the undue stimulus given to trade by a too free supply of borrowed capital from the Banks. In 1866, I wrote as follows:—"Admitting, as I freely do, that a convertible currency cannot exceed in amount the natural wants of a (healthy and legitimate) trade, it must also be asserted that these natural wants may be unnaturally, and therefore unhealthily, stimulated by an over-issue of currency. Those who argue to the contrary, seem to me to forget that, while the notes so over-issued will return, *the capital thereby advanced will remain*, as the issuing Bank will most certainly learn, on an early day, in its

exchanges with other Banks; and it cannot surely be denied that every advance, however made by a Bank, will be just so much additional capital thrown upon the market; and the supply of capital being thus augmented, its value will fall, prices will rise, and speculation will be encouraged."

And the way in which this operated I explained as follows:—

"When capital is plentiful, and therefore cheap, credit is also good. Capitalists are anxious to lend. Merchants are willing to borrow, in order that they may buy. Trade flows briskly; large quantities of goods are manufactured and exported, and large supplies of all kinds of foreign commodities are imported. Capitalists, not content with employing their money in this country, send it abroad for investment in foreign loans and 'securities.' Every one engaged in trade is prosperous, or thinks himself so, for profits are large; personal and family expenses increase accordingly; country mansions are built and equipages started; and consumption of all products, home and foreign, goes on at a much greater ratio than formerly. All sorts of schemes—some of them sound, many of them visionary, but all demanding large supplies of capital—are set afloat; shares are eagerly sought after, and are quoted at a premium; and, for a time, everything wears the aspect of ease and prosperity.

"During all this time, however, a draining of the national resources has been going on, quietly, but steadily; the surplus floating capital of the country is being rapidly used up, and is becoming, every day, scarcer and scarcer. Foreign markets are getting glutted, goods are not saleable abroad, and the capital invested in them does not return as it formerly did. Stocks of foreign goods at home are now also in excess, and hang heavy on the importers' hands. Wages have risen, and profits are diminished. The home consumption, however, still continues for a time at its former extravagant rate. And as the result of the whole, a heavy score is being run up against this country with foreign nations, which cannot be paid with goods, and must therefore be settled in gold."

This brings us face to face with the vexed question of foreign exchanges. By an adverse foreign exchange we mean, that there is a balance of payments due by this country to foreign countries, and it is therefore an infallible indicator that this country has been overtrading, or living beyond its means, or lending too much to foreign countries, or sinking too much capital in railways, or buildings, or other permanent erections, or that it has experienced the calamity of a bad harvest, for most assuredly each and all of these tend directly to turn the balance of account against us.

Thus—"A bad harvest is not only a loss of English capital, but it necessitates an import of foreign corn. Extravagant living not only consumes capital, but runs up a score against us with foreign nations. Over-trading absorbs capital in the manufacture or purchase of goods, and gluts the foreign market with commodities which are not, for the time, exchangeable. Building mills and constructing railways turn floating into fixed capital, and thereby just so much diminishes our power of barter with other countries. Lending money to foreigners, or investing in foreign securities, detracts, of course, from our capital at home by sending it abroad; or it converts a foreign debt, payable on demand, into a fixed loan, payable only at a future period; and so, in every way, the balance of account is turned against us, and must of necessity, as I have said, be paid by an export of gold."

But it is also certain that these same causes, which have brought about an adverse balance of trade, and consequent exportation of gold, have all along been operating, as I have already remarked, towards a draining of the national resources, so as to produce a scarcity of loanable capital in the country; for by their means it is evident that the floating, and therefore the loanable capital, has been locked up, or consumed, or sent out of the country, or fixed, or wholly lost, in a greater proportion than it has been replaced by the surplus profits of the country. After a time, therefore, we begin to find that "everything is tending in the opposite direction from that in which it was formerly moving—the tide has fully turned, and a strong ebb is running. 'Money' has become *perceptibly* dearer, and the augmented price begins to press hard on the merchant trading on credit and borrowed capital; foreign remittances are not forthcoming, and goods must be sacrificed to meet obligations; prices are falling, and profits are nowhere, or turned into losses; railway bills are abandoned, or the operations minimized; speculation has reached its limit, and 'Bears' are in the ascendant; scheme after scheme collapses, and 'premiums' are turned into 'discounts.' Banks and other capitalists take the alarm and button up their pockets. 'Money' gets dearer and dearer still, and so the drag is pressed down more and more heavily on the wheels, until, after a time of rude jolting, and some perturbation, and mayhap a few contusions, and even, it may be, broken bones, the speed is moderated, and the commercial machine moves on more slowly and more smoothly, and the frightened passengers recover breath, and find time and nerve to look about them, and to adjust themselves to more rational travelling.

"But not only does dear 'money' thus naturally press upon and discourage those very operations which have produced it, but it also

acts powerfully in promoting a return in the opposite direction. It not only puts on the drag, *it helps to reverse the engine,* for dear 'money' in England invites and provokes investment of foreign capital. The foreign capitalist, seeing English stocks, shares, goods, depreciated in price, steps in as a purchaser; or, tempted by the high rate of interest prevailing on this side, sends over capital for the purchase of long-dated English bills of undoubted character; and thus he materially assists in equalizing the foreign account, in easing the 'money' market, and in turning the exchanges in favour of England."

The same causes, then, we see, produce equally (or at least should, if allowed freely to operate) a drain of gold, and an enhanced value of loanable capital, or, in other words, a rise in the rate of discount. Now, there is nothing about which all commercial men are so thoroughly agreed, as that cheap capital fosters trade, and also all sorts of speculative transactions; and that dear capital discourages them. And there is nothing about which all, or nearly all, financial men are so well agreed as that the true specific for an adverse exchange is a rise in the rate of discount; for this operates towards a cure, as we have seen, in two ways,—first, as a drag upon the high pressure speed of inflated trade and rash speculation; and, second, as an inducement to foreign capitalists to invest in English securities.

That is to say, the same causes which produce the *bane* operate also towards the production of the *antidote;* or more correctly, the effects produced, in the first instance, have within themselves a natural tendency to counteract and nullify those very causes which have produced them, just as the excess of speed of the steam-engine increases the action of the "regulator," which, in its turn, operates towards reducing the speed which has set it in motion.

In all things, natural and physical, there are conditions of equilibrium towards which all disturbances tend to return; and such, I believe, is the case also in the commercial world. And not only so, but I believe it to be the law of all such disturbances, that the farther they carry themselves beyond the condition of equilibrium the more increasingly powerful are the counteracting forces which are set in motion; just as the higher the pendulum swings beyond the state of rest, so much more powerful are the forces which first retard its progress, and then prevail to send it back again.

Now, under a system of purely metallic currency, the natural operation of these causes could not possibly be interfered with. A drain of gold must at once act upon the reserves of the Banks, and

will so far diminish their power of issue: prudence will therefore hint at caution in lending. If the drain continues, the reserves will be still more depleted, and advances must be further curtailed. This of course withdraws capital from the market, and, as a consequence, enhances its value. As the process goes on, the power of making advances is more and more restricted, more and more capital is withdrawn, and the rate of interest continues to rise, until, by the withdrawal of capital and consequent enhancement of its price, inflated trade and speculation are alike discouraged, the adverse balance of payments is redressed, the drain is arrested, and the tide begins to turn the other way. This, it is evident, would be the natural process in the case of a currency purely metallic; and we conclude that it *ought to be* the same in the case of a mixed currency of gold and paper, otherwise the corrective referred to will not be applied. That is to say, if, when an adverse exchange has taken place, and gold has begun to leave the country, the process by which, as we have seen, the price of loanable capital becomes, under these circumstances, naturally and inevitably augmented, should be interfered with by an over-issue of currency acting upon the supply of capital in the market, the consequence will be that the high-pressure speed will continue, and ruin and disaster will follow.

To recapitulate. We have seen that, under a mixed currency, where there is no restriction upon the issue of the paper, there exists a great temptation to the issuer to give out more of his paper than is prudent; that the effect of such an over-issue is, first, to depreciate the value of the entire currency in relation to foreign currencies, and so to lead to an export of gold; and second, to throw upon the market more borrowed capital than a legitimate and healthy trade should require—the effect of which is unduly to stimulate trade and to foster speculation, both of which again naturally produce a balance of payments against this country, indicated by an adverse exchange, and followed, as before, by an export of gold. We have seen further, that, by the consent of all financial authorities, the only cure, whether for a depreciation of the currency or for an adverse balance of payments, is a rise in the price of loanable capital—in other words, in the rate of interest. And we have found that, under a system of purely metallic currency, this corrective would be naturally and inevitably supplied, but that, under a system of unrestricted mixed currency, the temptation, already referred to, might, and almost certainly would, interfere with the operation of this natural and thoroughly efficient corrective. Hence we conclude, in the words of Mr. Lowe, "that a mixed currency must be at all times

exactly of the same amount, and consequently of the same value, as a purely metallic currency would be."

Mr. Lowe adds,—"The plan which you propose does not fulfil the third condition, and appears to me therefore inadmissible." This would seem inevitably to follow from the considerations already adduced, but there is another consideration which the gentlemen of the Chamber of Commerce appear to me to have overlooked, and which I have not hitherto noticed, because I did not think it necessary to my argument, and that is, that, were their utmost desires gratified by the unconditional repeal of the statutes objected to, they could not possibly have the same security for the issues of any new Bank which might thereafter be started, as they have now for those of the existing Banks; because, most assuredly, no new Bank could now be floated, except on the principle of Limited Liability; and can it be supposed that the notes of such an establishment would be received with the same confidence as those which are guaranteed by the entire means and substance of every shareholder of the Bank which issues them?

I have thus endeavoured to state the reasons which have led me to adopt the views enunciated by Mr. Lowe. They are the same reasons which I offered for the consideration of the Chamber of Commerce in 1866; and, on looking back on what I then wrote, I do not find that I have now anything material to alter. Understand, however, that I am not an out-and-out supporter of the Bank Acts. I pointed out at that time some practical mistakes, which, in my opinion, the framers of these Acts had made, and to these objections I still adhere; but I have nevertheless no hesitation in expressing my entire faith in the *principles* on which these Acts profess to be founded, as well as my approval of the purposes which they seek to accomplish.

APPENDIX.

OPINION OF SIR R. PEEL.

"I say that a promissory note, payable to bearer on demand, is of a totally different character from all those other descriptions of currency, and that it has a totally different effect. In the first place, the nature of the instrument is different. It performs the office of a currency; it passes from hand to hand; no personal guarantee is required; it acts as a substitute for coin; it is not like

a bill of exchange, which requires an endorsement at every transfer; it is a substitute for money; it performs the functions of money, and it acts on prices as money does."—*Speech of* Sir R. Peel, *May* 6, 1844.

MR. TOOKE AS TO FREE TRADE IN ISSUES.

"Whatever may be determined upon as to the expediency of providing security for the general circulation of the country Banks, there is one part of that circulation which ought not upon any footing, or with any modification, to be any longer tolerated. I mean the notes under £5. These are in every point of view a most objectionable medium of exchange. Leaving out of the question the consideration of fluctuations in the value of the currency, some protection of the lower classes against the severe losses to which they have so often been exposed, as the holders of small notes, when the Banks issuing them have failed, is imperatively called for. It is quite idle to say that the lower classes have the option of refusing to take the country notes. Practically, in the great majority of instances, they have not and cannot have that option. But if there is any subject more important than another, for which the Government of every state has been invested with the privilege of coining money, it is that of protecting the lower classes of society, who are little competent in this particular to protect themselves, from the risk of loss in receiving their stipulated wages or other payments. And this function of the sovereignty of issuing coin, which, under the strongest sanctions, certifies to every member of the community the weight and fineness of the money, when he may be entitled to receive it, is justly guarded against the interference of private individuals by the severest penalties. But by a strange inconsistency, while such care is manifested to secure the community from being imposed upon by metallic money of less than standard value, every adventurer who chooses to follow the trade of a banker is allowed to issue a spurious paper money, which, having neither intrinsic value nor any adequate security for eventual convertibility, is in too many instances neither more nor less than a fraud—a fraud of which the most numerous and helpless classes, and those who can least bear the loss, are most commonly the victims."—(*Consideration on the State of the Currency*, 1826, pp. 127, 128, by Thomas Tooke.)

"At the time when these remarks were written, the amount of the circulation of one pound notes in England and Wales had been computed at upwards of five millions. *The evil, therefore, of the suspension of so many issuing Banks,* as had then recently failed, was one*

* These, it is right to say, were Private Banks.

of great magnitude, which fully justified, or rather demanded, the interference of the Government and the legislature to provide a remedy; and this was most effectually done by the suppression of that objectionable class of notes."—*An Inquiry into the Currency Principle*, by Thomas Tooke, Esq., 1844.

"As regards the issue of notes, there can be no doubt that it comes distinctly within the province of regulation by the State. With regard to the other functions of the Banks, they do not come necessarily within the cognizance of the Legislature; but, in as far as they require, for the convenience of management, certain facilities and privileges which can only be conferred by Parliament, they become amenable to such restrictions and regulations as, in the view of the public interest, the Legislature may think proper to impose as the condition of the privileges conferred."—Tooke's *History of Prices*, vol. ii., p. 310.

M. RICARDO AS TO DEPRECIATION OF THE CURRENCY.

"If a mine were discovered in any country, the currency in that country would be lowered in consequence of the increased quantity of precious metals brought into circulation, and would therefore no longer be of the same value as in other countries—gold and silver, whether in coin or bullion, obeying the same law which regulates all other commodities. If instead of a mine being discovered in any country, a Bank were established, such as the Bank of England, with the power of issuing its notes as a circulating medium, after a large amount had been issued either by loans to merchants or by advances to Government, thereby adding considerably to the sum of the currency, the same effect would follow as in the case of the mine. The circulating medium would be lowered in value, and goods would experience a proportionate rise." "If an addition be made to a currency partly of gold and partly of paper by an increase of paper currency, the value of the whole currency would be diminished, or, in other words, the prices of commodities would rise, estimated either in gold or in paper currency. The same commodity would purchase, after the increase of paper, a greater number of ounces of gold coin, because it would exchange a greater quantity of money."—*M'Culloch's Edition*, p. 264.

MR. FULLARTON AS TO COMPETITION OF BANKS.

"A more remarkable and decided test, however, of the utter powerlessness of Banks to increase the issue of their notes *ad libitum*, has been supplied by the history of our currency, from the period of the

first introduction of Joint-stock Banks in 1833 to the present time. These gigantic establishments were scarcely set on foot when they entered on a career of the most uncompromising competition with the private Banks, and recently established branches of the Bank of England, and prosecuted that competition by the most unscrupulous means. So systematic, determined, and powerful a series of efforts *to enlarge more particularly the circulation of Bank-notes* had probably never been attempted since the invention of banking." "By the facilities which they thus afforded to wild speculators, men without capital of their own, *they contributed largely to the calamitous reverses which soon followed.* They succeeded also, to a considerable extent, in driving weaker or less venturous rivals from the field, and *in usurping a more than fair share of such additional issue of Bank-notes* as the stir of industry, and the extension of production between 1833 and 1839 had called into existence."—*Mr. Fullarton's* "*Regulation of Currencies,*" p. 89.

N.B.—Mr. Fullarton was a strenuous opponent of the Bank Acts; so also was Mr. Tooke.

THE AGRICULTURAL AND COMMERCIAL BANK OF IRELAND.

THIS Bank was started, I think, about the year 1834, and came to a disastrous conclusion about the year 1838. Their Capital was divided into small shares, so as to enable people of but very limited means to become partners. Their management was very liberal. They gave a higher rate of interest on deposits than we did, and they were more free in their discounts; for they were in some instances above the old prejudice which counselled attention to the genuineness of the signatures in the Bills offered to them. The great characteristic, however, of their management, was *their desire to circulate the notes of the Bank.* With this view small dealers got large credits which ended badly for them, as well as for the Bank. Further, their friends were supplied with their notes for the purpose of exchanging them for others. There was thus no restriction on the issue of Bankers' notes in Ireland, and the country, whenever they had an opportunity, was deluged with their notes. The derangement to the legitimate business of Banking was not the only evil of this forced circulation. *Men of character and substance were, in many instances, almost driven out of business through the competition of men having neither, but to whom, in order to promote such a circulation, the Bank gave advances which they knew not how to use.*

There was thus no limited liability, and the noteholders were, after some suspense and delay, paid in full; but the calls made after

the stoppage of the Bank fell very hard on very many, and caused not a few to leave the country.—*Extract from Private Letter from a Banker of forty years' standing, now holding high office in an Irish Bank.*—15*th March*, 1873.

XX.—*The Mint Bank Note of the Future.* *By* MR. J. ANDERSON.

Read before the Sanitary and Social Economy Section of the Society, March 17, 1873.

MR. GEORGE ANDERSON, the junior member for Glasgow, in his letter to the Glasgow Chamber of Commerce, says,—"I entirely fail to see any necessary connection between the trade of banking and the power of issuing money. I look on the power to issue money, whether coinage, or notes, as properly a State prerogative; and I know no argument in favour of allowing private citizens, whether bankers, or otherwise, to issue notes that would not equally apply to allowing them to have a coinage of their own. You will understand from the foregoing, that my mode of getting quit of banking monopoly would be, to take away from bankers the power of issue, which they at present improperly possess, and which is the only thing that gives them any monopoly. Abolish that, and banking is at once free. The Chamber of Commerce proposes merely to enlarge the sphere of the present abuse. If the prerogative of issuing money were restored to the State, the use of the notes, so issued, ought to be freely obtained by any banker who deposited the necessary security in State stock."

I have long held opinions similar to these, and as far back as 1865, I got a specimen note made, and a few pages printed, shewing how the idea could easily be carried out. The method is very simple. Let the Royal Mint be constituted the government office for the supply of the national currency, whether in gold, silver, bronze, or paper. Let any one wishing to get notes, go to the Mint with his Government stock, and get Mint notes for it, in the same way as he now goes with his bar gold, getting sovereigns in return. If he is a banker, and wishes to issue these Mint notes to the public, let him place, on the other side of the Mint note, his obligation to pay the bearer, on demand, the value of the note in gold. Let us examine, and see whether this note performs Mr. Lowe's three conditions. The first

of these is "the paper must be convertible into gold on demand." At present, all the banks that issue notes come under this obligation. Let the same obligation be continued by all who issue Mint notes. As long as the issuer is solvent, this obligation will be fulfilled. At present, should a bank fail to implement this obligation, the holder will sustain the loss; but with the Mint note, should the bank fail, the holders would fall back on the Mint, and claim the Government stock lodged when the note was issued. In this manner, the holder of the note is doubly secured, both bankers and Government must fail, before he can lose the entire value of his note.

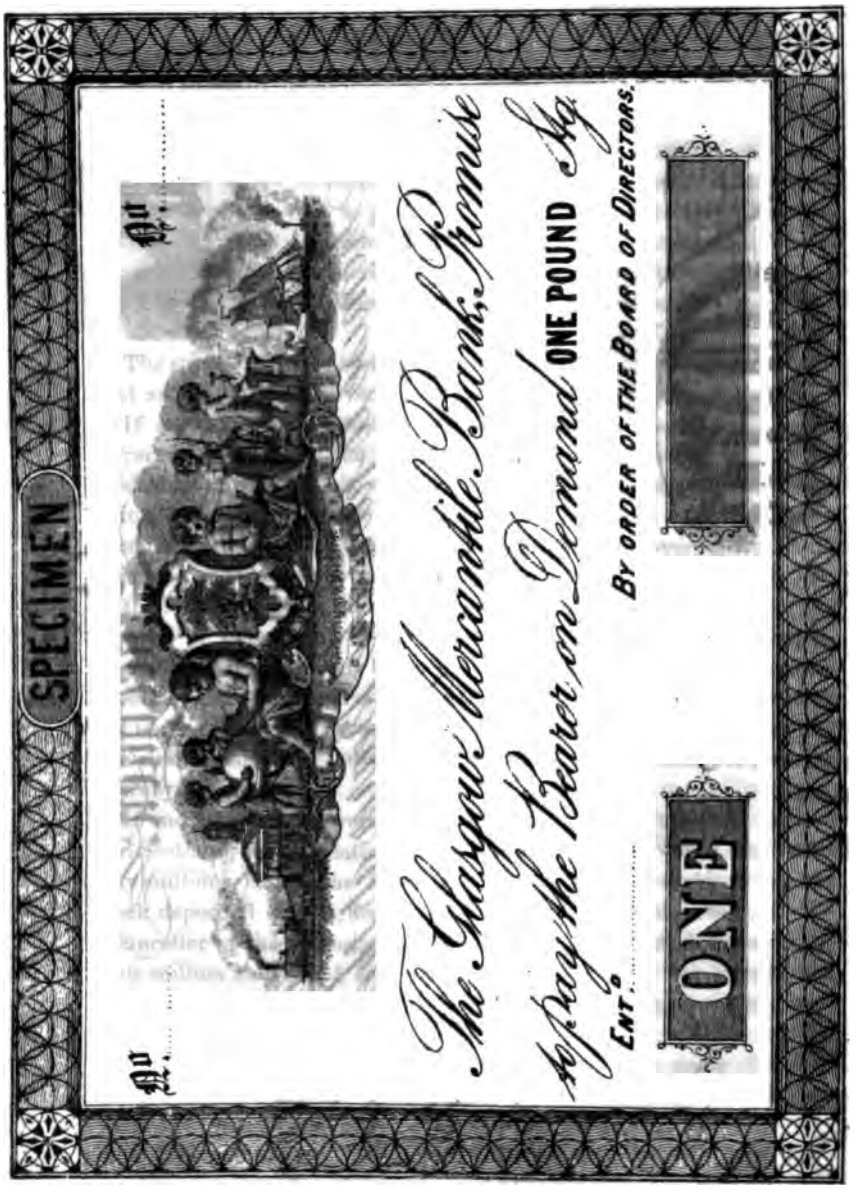
On account of its simplicity, I have supposed that the banker pays his pound in gold, and the Mint its pound in consols.

At present a pound of consols is not worth a pound sterling. You can buy £100 consols for about £92 sterling, or about 18s. 5d. for a pound of consols. Suppose then, that with consols at their present value, a banker issuing the Mint note were to fail; if he paid 1s. 7d. in the pound to the holder of the note, the 18s. 5d. from the Mint would make 20s., so that the holder of the Mint note would be paid in full. Seeing that hitherto, all Scotch bank notes have been paid in full, the chance of any bank paying less than 1s. 7d. is very small.

The conveniency of paper currency is so great, that the public would not hesitate to take this very small risk. The Scotch public have been willing, not only to take the entire risk of the banks, but actually to supply them with the gold, or capital, on which the Scotch currency is based. As an illustration; let a customer go to a Scotch bank with 200 sovereigns, let him ask a deposit-receipt for one hundred, and a bank note for the other hundred. Three months after this, let him return to the bank, and demand his gold. On presenting his deposit-receipt, he will get his 100 sovereigns, and the interest, which accrued on them for three months. On presenting his note, he gets his bare 100 sovereigns, the bank appropriating to itself the interest on the latter. In this manner, the Scotch banks manage to appropriate to themselves the use of about three millions of the people's capital.

The only benefit I know of, that has been derived from the Act of 1845 is, that it has prevented the Scotch banks acquiring any vested rights to the increase in the circulation of notes, since the passing of that Act, and has reserved to the nation, the undoubted right to about three millions of the present circulation.

Mr. Lowe's second condition is, that "sufficient security must be held by the issuer, to secure the payment of the note." What I have already stated is so far a fulfilment of this condition, with this differ-



difference between consols and gold, and this would require to be effected through the issuer, who promised to pay the notes in gold.

held by the issuer, to secure the payment of the note." What I have already stated is so far a fulfilment of this condition, with this differ-

ence, that the security is not held by the individual who issues the note, but by the Mint.

Mr. Lowe's third condition is, "the mixed currency must be at all times exactly of the same amount, and consequently of the same value, as a purely metallic currency would be." That it should be of the same amount as a purely metallic currency is very doubtful, because paper being much handier, and more convenient than coin, it would be used and carried about in larger quantities. The paper currency being at all times convertible into gold, would, of course, be of the same value.

The supply of notes, by the Mint, would be regulated by the demand, just as the supply of sovereigns now is.

If a banker wished to increase his supply of notes, he would send government stock to the Mint, and get notes in exchange. If he wished to diminish his stock of notes, he would take them to the Mint, and get his consols back again; or a banker might open an account with the Mint, and deposit an excess of government stock, over and above what covered his Mint notes; the Mint debiting him with the notes issued, and appropriating stock, or interest, only to the amount of notes at his debit. This would give the banker an opportunity of carrying on his business, with the smallest number of notes possible, and afford him an opportunity of expanding, or contracting his issue as he found necessary. There would be no tendency to force the circulation of the notes, as the banker would gain little, or nothing thereby.

If one pound notes were issued in England, the same as in Ireland and Scotland, the circulation of the United Kingdom would soon reach fifty millions, and if the Mint were to receive the dividend on the stock deposited with it, for the notes issued, it would return, to the Chancellor of the Exchequer, a million and a half sterling annually. This million and a half, as I have shewn, belongs to the people, and not to the banks; and the Chancellor of the Exchequer is, in duty, bound to appropriate it to the benefit of the nation.

The great objection to State paper currency is, its susceptibility of being depreciated by the operations of the Chancellor of the Exchequer, when he is hard pressed by the exigencies of the nation.

If this system were in operation, it would require some ingenuity, on the part of any Chancellor, to depreciate it; and for this very evident reason—every note in circulation is already pledged to nearly its full value; the only margin, left to be operated upon, is the difference between consols and gold, and this would require to be effected through the issuer, who promised to pay the notes in gold.

If the Mint note is not made a legal tender, it cannot be depreciated; and no bad consequences are found to result, in Scotland, from its paper currency not being a legal tender. The Scotch bank notes have been taken by the people, as readily in Scotland, as the Bank of England notes in England, although the latter are a legal tender, and the former are not. You will also observe, that it is not the Government that pays gold for the Mint note, but the banker who issues it, and, as long as the note is payable in gold, on demand, it cannot be depreciated. What possible good, then, would it do to any Chancellor of the Exchequer, to relieve bankers of this obligation?

In this arrangement, we have the combination of two systems, each of which has its own advocates. We have a uniform State paper currency, all over the nation, combined with a local, private bank issue. Mr. Mason, very justly, objects to the Government being dealers in gold. In this arrangement, the traffic in the precious metals is left entirely to the bullionist and banker. I do not agree, however, with Mr. Mason, when he says, "that system (of currency) which is perhaps the most popular, is, to allow all banks freedom to issue notes against Government securities. This plan has always appeared to me objectionable, because you would most surely foster weak banks, and entrap people to do business with them by the fancied security which the notes would give to the public."

Because there are always a few individuals who will act either simply or recklessly, Mr. Mason advocates, on their account, the continuation of the injurious restrictions upon trade and commerce, and the treatment of the whole commercial community, as if it were simple or reckless—a policy which has long since been abandoned, as contrary to the principles of political economy.

A distinct line being thus drawn between currency and banking, the legislation on the subject becomes simple and easy.

As soon as the Mint is prepared to issue these notes, against Government stock, let all the banks in the kingdom be relieved from all legal restrictions, and let them have perfect liberty to trade in capital, the same as any other merchant has to trade in goods.

XXI.—*Technical Education.* *By* MR. DAVID SANDEMAN.

Read before the Society, April 9, 1873.

THIS subject has formed a topic among us for some years past, but has not as yet been taken up in that practical manner in which our countrymen usually go to work in our great social movements. The apparent indifference may be accounted for in two ways. *Firstly*, The people generally are not thoroughly alive to its necessity. *Secondly*, The uncertainty which prevails in the minds of those who admit its necessity, as to the best means of setting about it. I need scarcely repeat here the evidence which is to be found in the Blue Books, and given by the Jurors from this country, specially requested by the Schools Inquiry Commission to report upon the progress made by our Continental neighbours upon their various industries, as shewn by them at the Paris Exhibition of 1867, in comparison with our own.

Professor Tyndall, Mr. Huth, Mr. Mallet, Dr. Lyon Playfair, Mr. M'Connell, Mr. Ripley, Mr. Young of Kelly, Mr. Mundella, Mr. Platt, Mr. Bragg, Dr. Frankland, The Rev. Canon Norris, Mr. Scott Russel, &c., &c., together with many of our Chambers of Commerce throughout the country, all unite in bearing testimony to the rapid strides which our competitors in France, Belgium, Germany, and Switzerland are making upon us, in the greater number of our staple industries, and which they attribute to their excellent system of Technical Instruction.

Two years ago, the Secretary of the Birmingham Chamber of Commerce sent me a list of above 100 different articles, manufactured in their district, which they formerly exported largely, but in which they are now either very strongly competed with, or entirely supplanted in, by other countries.

I may also state my own experience of the branch of trade in which I have been engaged—viz., that of "*Woollen Yarns.*" About thirty years ago, the manufacturers of Glasgow were almost wholly supplied by the spinners of Yorkshire. Now the very reverse is the case,—they are mainly dependent for their woollen yarns upon the spinners of Belgium and the Rhenish provinces. It is difficult to get reliable statistics of the proportion which the English spun yarn

bears to the foreign; but I may venture to give an opinion, that not one pound of English yarn is now manufactured in Glasgow for every hundred of foreign. The cause of this cannot be attributed to the Belgians and others having greater facilities for procuring the raw material, the wool chiefly employed in the production of the yarn being imported from Buenos Ayres, generally in British vessels, and sold by auction at Liverpool, Havre, and Antwerp, for British account, so that our spinners can purchase it on fully as favourable terms as their competitors. Owing, however, to this wool being very *burry*, our spinners prefer paying more money for a wool which is free from this fault, and thus neglect an article which their more technically instructed competitors secure at a comparatively low figure, and by careful manipulation are enabled to turn to profitable account. It is by this means they have acquired (what I scarcely exaggerate in describing as) a monopoly of the trade.

The same remarks apply with still greater force to Angola yarns, a mixture of wool and cotton, supplied to the Glasgow manufacturers by Saxon spinners, who, although working under the disadvantage of buying both wool and cotton to a very large extent in England (having thus the extra carriage to pay), not only compete successfully with English spinners, but, by their superior skill, produce a yarn which our home spinners have hitherto been unable to approach as regards excellence of manipulation.

Instances of this description can easily be brought forward in many other branches of industry; but I may be allowed to state, that the manufacture of scarfs, which was carried on to a considerable extent in Glasgow, has lately disappeared: it has been completely superseded by the superior styles of French and German manufacturers. A very intelligent German, who established himself in Glasgow some years since, for the purpose of buying dress goods here for Continental markets, assures me that he is *alarmed* at the progress in design and execution recently shewn in the goods produced by the looms of Saxony and France, which can only arise from the superior training in Technical Instruction, as in the case of Saxony the manufacturer frequently buys all his yarn in England, and in that of France a considerable proportion of it.

I know that many consider the apprenticeship in a workshop or factory superior to any instruction which can possibly be given in a Technical Institution. I do not intend to deny this, provided it was the duty as well as the interest of any one in the factory (*and who was at same time competent*) to give such instruction; but there is no time for an employment of this description: there is too much strain and

pressure in business to permit any foreman to teach; and the result is, that even in cases where high fees are paid by what are known in the various industries as "information men," no one instructs them, but, on the contrary, actual barriers are not unfrequently put in their way.

I may now be permitted to state that the countries which are our most powerful competitors in arts and manufactures are those which have devoted the greatest attention to Technical Instruction, while even those whose competition does not yet affect us are fast following their example. I have no intention of wearying my readers with statistics, but may be allowed briefly to refer to what is being done on the Continent. In *Finland*, the enthusiasm for science burns brightly; at Helsingfors, the extensive buildings devoted to chemistry, natural history, and natural philosophy, are found to be inadequate, and a new edifice is rising to its third story, for the better accommodation of the polytechnic department of the flourishing Finnish University. In *Russia*, Technical Education is widespread: the Technological Institute of St. Petersburg now ranks among the most successful Technical Institutions of Europe. Regarding *Prussia*, allow me to quote from the Report of Commissioners sent by the French Government to inquire into the state of Technical Instruction in Germany, &c.:—"Generally, all the information collected respecting the organization of instruction in Prussia shews that, during the last forty years, the government has made the most liberal efforts to extend and diffuse the education required by the different classes of the community. Primary instruction has been made general, and has been improved; the middle-class schools, the industrial schools, the special weaving schools, the practical schools, the schools of architecture, of mines, and of navigation; the gymnasia, the polytechnic institutes, and lastly, the universities, form a complete whole, amply sufficient for the present requirements of public instruction."* The middle-class schools give mathematics and other branches of science, and the modern languages. The industrial are equivalent to our ideas of schools of art and manufacture. The special weaving school deserves a detailed description, for which purpose I will select that of *Mülheim*, on the Rhine, at which several young Scottish manufacturers have taken instructions. The course comprises—

Origin and History of Raw Materials—wool, cotton, silk, linen, &c.

Spinning and twisting (theory of).

Dyeing.

Arrangement of the raw materials for weaving, winding, spooling, warping, &c.

* See Gill's *Pamphlet.*

Decomposition of textures.

Patterning, inventing, and putting together of patterns.

Calculation of goods according to patterns.

Hand-looms—their arrangement for treadle and Jacquard-weaving, in tweeds, cotton, linen, silk, and mixed materials.

Power-looms—their mounting and driving.

Practical weaving of all kinds of yarn on both looms.

Practical dyeing, bleaching.

Theory of pressing and finishing of raw and woven materials, in connection with chemical lectures and experiments.

Drawing from patterns, to be executed with hand as well as power-looms.

The students have at their disposal—

Sixteen hand-looms, all on different principles.

Six hand-looms for carrying out self-invented patterns.

Eight power-looms, driven by a four horse-power steam engine.

Three ribbon looms.

Card-cutting, warping-mills, and other preparatory machines.

A well-arranged dye-house; chemical laboratory.

The arrangement admits of pupils entering at any time; and the school, having taken for its principle to form practical men, teaches only that which is necessary and will be of use to each: the student is not compelled to go through all the branches, but only those which he finds suitable for special requirement. The fee for the whole course, which may be completed in one year, is £13, 10s.; the materials extra, about £3.*

An institution of this description would be a valuable assistance to our manufacturers; and I would respectfully recommend its being among the first steps of our Technical movement.

The School of Architecture embraces two years:

1st year—Construction Exercises.
Study of Machinery used in Practical Architecture.
Theory of Architectural Construction.
Ornamental Drawing.

2nd year—Architecture as a Fine Art.
Exercises in Architectural Drawing.
History of Architecture.
Ornamental Drawing and Modelling.

The schools of mines and navigation are similar to those of our own country.

The gymnasia on a par with our higher classical schools or colleges.

* See Mülheim *Prospectus*.

The polytechnic institutes are in technical studies equal to the universities as regards classical studies. The same or similar courses obtain over Europe; but I may remark that Prussia, ever alive to the necessity for moving forward, has been modifying its system, and to some extent adopting the Latin proverb, "*Fas est et ab hoste doceri.*" They feel the truth of Monsieur Montjean's remarks, who "thinks that no German school comes near the Central School of Arts and Trades in Paris; that the too practical system of the German schools—the prevalence of an instruction based *entirely* on the applied sciences—has directed the attention of young men *exclusively* to subjects of self-interest and immediate profit, and has quenched the generous aspiration which sees in life something else than the mere profit of trade to be realized. The moral is, in fact, sacrificed to the mechanical man."* They are consequently introducing ethics and political economy, and generally raising the moral standard. As Monsieur Duruy expresses it,—"While the lessons in the applied sciences give a practical direction to the student's mind, the lessons in history, literature, and ethics will instil into him the desire to rise above the realities of the physical world."

Dr. Lyon Playfair, in his excellent "Lecture on Technical Education," in Edinburgh, two years ago, dwells upon the success which has attended the Technical Institutions of Switzerland and Holland. He says:—"What has enabled this little nation (Switzerland), so remote from the pathways of commerce, and so poor in the mineral resources of industry, to carry on manufacturing production by the aid of a prosperous and contented people, while England, washed by the ocean and abounding in mineral wealth, is burdened with an ever-increasing proportion of the unproductive poor? There is only one answer, that Switzerland has a highly-educated people." And of Holland he adds,—"Despite her natural poverty in the raw materials of industry, Holland sends to this country alone exports of food to the annual value of £5,000,000, and manufactured products worth £6,000,000 more. The law compels every town of 10,000 inhabitants to erect Technical Schools." Among Dr. Playfair's conclusions, he states "that a higher education, in relation to the industries of the country, is an essential condition for the continued prosperity of the people; for intelligence and skill, as factors in productive industry, are constantly becoming of greater value than the possession of native raw material or local advantages."

The great Republic of America fully recognizes the importance of Technical Instruction. I quote from a Report of Drs. Machattie and

* Gill's *Pamphlet.*

Hodgins, who were deputed by the Province of Ontario, in Canada, to proceed to the United States, for the purpose of inspecting and reporting upon any Technical or Science Schools or Colleges there established. This Report, dated Toronto, January, 1871, has been kindly handed to me by Dr. Machattie, who is at present in Glasgow. It confines itself to eight of the principal Colleges, but states that there are other fifty Institutes which have scientific departments.

These eight cost about . . . . . £350,000 in erecting.

Revenue, . .	Fees from students, about		20,000
	Other sources,	„	40,000
Expenditure,	Salaries,	„	44,000
	Other expenses,	„	16,000

With eighty-three Professors, and fifty-five Instructors, and 2,200 Students, all above sixteen years of age.

"The course of study varied. They all, however, embraced Mathematics, Chemistry, Natural Philosophy, Drawing, Civil and Mechanical Engineering, and the Modern Languages,—*so many of the Science Text-books and Works required to be used and consulted being written in French and German.*

"Fees we found to average £30 a year; in Cornell University, £9. A first supply of apparatus and chemicals was usually given to each student; subsequent supplies had to be paid for, at about cost prices, while breakages were at the risk of the student, who was required to pay for them. Technical Schools in the United States have been an insured success: they must be kept entirely apart from and independent of any other literary or scientific schools or colleges. To this point we would most particularly draw attention, for we consider it one of the chief essentials to success. A school of Technology should, in its teaching, management, and government, be kept entirely distinct from any other institution. Even at the two Universities of Harvard and Yale, where scientific schools exist, their efficiency and success are just in proportion to their entire practical separation, for teaching and other purposes, from the other parts of the University."

In urging the necessity for a School of Technology in Ontario, Messrs. Hodgins and Machattie conclude in words which we may take home to ourselves:—"We have hitherto been content to receive our supply of such skilled assistance from abroad. In this respect our American neighbours furnish a favourable contrast. We must, therefore, provide liberally for the practical instruction of our youth, in every grade and department of knowledge, so that, with God's blessing, we shall not fall behind in the great race of national intelligence and progress."

I would strongly recommend a careful perusal of this interesting Report to all who desire success to the Technical movement in Glasgow: it is full of excellent practical hints; and I regret that my space prevents further extracts from it. I may be permitted, however, to remind my readers that, by an Act of Congress in 1862, "a quantity of public land was apportioned to each State, equal to 30,000 acres, for each Senator and Representative in Congress, according to the census of 1860. The object of the grant was to provide in each State of the Union for "the endowment, support, and maintenance of at least one College, where (without excluding other scientific and classical studies and military tactics) such branches of learning should be taught as are related to agriculture and mechanical arts, in such manner as the Legislatures of the States may respectively prescribe, *in order to promote the liberal and practical education of the industrial classes in the several pursuits and professions of life.*" Hence, at the Worcester Institute of Industrial Science, out of the eighty students in attendance, only seven of them paid an annual fee of £20—the other seventy-three were admitted free." Dr. Machattie informs me that the States go on increasing such institutions, and that Canada is not less zealous in the cause.

I have expatiated rather freely upon what is doing, and has been done, on the Continent and in America; but I am well aware that in our own country we also are not passive.

In Art, since 1851, we have advanced in a very marked degree, the great Exhibition of 1862 having proved that the Faculty of Design is as prominent in our countrymen, when properly developed, as in our more technically instructed Continental neighbours.

The Science and Art Department are ready to assist any district which shews its desire to establish Technical Institutions.

The Society of Arts promotes Elementary Drawing and Modelling Schools, and assists the establishment of Government Schools of Design, and, by courses of lectures, called "Cantor," does material service in advancing the industrial and commercial education of the people. It has proposed to hold examinations in the art and technology of the manufactures of cotton and paper; also for silk, steel, and carriages. They will be divided into three parts,—*First.* Those branches of science a knowledge of which is requisite as a foundation for sound technical instruction. *Second.* The technology of the manufacture, or the special application of the various branches of science to it. *Third.* The practical skill in the manufacture itself. These programmes may be had on application to the Secretary of the Society, John Street, Adelphi, London.

The Stationers' Hall is giving most instructive lectures, by Ellis A. Davidson, upon Printing, Type Founding, Paper-making, Engraving and Wood-cutting.

In Glasgow, we have the only Chair of Technical Chemistry in Great Britain, for which we are indebted to the munificence of Mr. Young, who has further views in a like direction in connection with that noble institution, the Andersonian University. I refer to the Chair of Applied Mechanics, occupied temporarily by Mr. Laurence Hill, and towards which £4,500 have been already subscribed for its permanent endowment. The Andersonian, besides, gives instruction to about 1,100 students in day classes, in Mathematics, Chemistry, Anatomy, Medicine, French, Latin, Greek, and Hebrew; and about 1,100 students in evening classes, in Natural Philosophy, Chemistry, Scientific Music, &c., &c.

The Established Normal School educates 171 pupil teachers, in whose examination this year there was not a single failure.

The Free Normal 149, with only one failure; and, in addition, above 1,000 children each, many of whom get a little science. In the Established Normal School, the female pupil teachers have dinner prepared by two of their number consecutively, which must form a very useful addition to their other acquirements.

The School of Arts is doing noble work; and it is much to be regretted that the premises which they occupy are not more suitable. In addition to 1,307 students, in 1872, of which 157 passed the examination, they give external instruction to about 230 students from the Free and Established Normal Seminaries.

The Mechanics' Institution, besides a goodly number of day students, had an attendance in the evening of about 2,300, shewing a considerable increase from 1870–71, chiefly in the mathematical, English, and Spanish classes.

Then there is the Bristol Trade School, which may serve as a model, shewing what a vigorous, well-trained mind like the late Canon Moseley can accomplish. At a moderate cost, one-third of which is contributed by the Science and Art Department, this school educated, in 1868, 132 boys—viz., 82 in the elementary and 50 in the science division. Fees began at 6d. and 1s. a-week, but are now £3 a-year for the science branch. They are taught Mathematics, Descriptive Geometry, Mechanical and Architectural Drawing, Mechanics, Experimental Physics, Chemistry, Laboratory practice, Steam, Geology, Vegetable Physiology and Botany; Freehand, Model, and Perspective Drawing, as applied to Design. An immediate connection can be observed between the *avocation* of the student and the studies which he has selected. A

mining school affiliated is now successfully working under Mr. Munro, formerly a Bristol Trade schoolboy.

Keighley, in Yorkshire, has recently established a very excellent trade school, for a detailed account of which I must refer to *Blackwood's Magazine*, Feb., 1872, and two pamphlets by its indefatigable Secretary, Mr. Swire Smith, especially in his *Educational Comparisons in England, Germany, and Switzerland.*"

There is also the Physical Science College, Newcastle, and others, all working towards the production of teachers for the Technical Institutions, which it is to be hoped will shortly be established, for the proper direction of that vast amount of intellectual wealth which at present lies dormant for want of scientific training and development. We endow Universities for classical, medical, and theological studies, and I am glad to find a disposition to favour the higher development of science in their halls, Trinity College, Cambridge, having recently elected two Fellows for their attainments in Natural Science. In Edinburgh University, also, they are now granting degrees in science. Dr. Playfair says—"The degrees in science, which some of our Universities think are unwise, and a modern innovation, were, in point of fact, recognized by our ancestors. In the organization of the Scottish Universities, after the Reformation, such degrees were distinctly referred to."

We support most liberally ten great military schools, besides a series of naval ones. If, therefore, we consider it necessary to educate our lawyers, doctors, and divines, and deem it essential to the maintenance of our high position, as one of the great powers of the world, that we should have naval and military colleges, is it not equally necessary to foster and strengthen, by every means, the source of that wealth which we so jealously guard, and upon which our prosperity depends, viz., the vast industrial arts and manufactures of our country? If we protect the casket, how much more seriously are we bound to care for the jewel within it.

I shall not yield to any one in my admiration of the practical intelligence and skill of the population of our country. Shew them the way to set about any work, and no foreigner will beat them in the rapidity and excellent manner in which they will execute it. But, as a rule, they cannot give a scientific reason for their system of manipulation, and, were they ever so willing, cannot consequently communicate their knowledge to others,—a knowledge only acquired after a practice of many years, and reaching perfection at a time of life when physical energy is on the wane —a knowledge which, had they received a scientific or Technical Education, they might have acquired ere they had reached the prime of

life. How would society tolerate such a state of things in a medical man, who, instead of beginning his career with the diploma, which recognizes him as qualified to undertake at once the duties of his profession, would, under the state of industrial education, remain comparatively ignorant of the principles the greater part of his life, and only acquire sufficient knowledge to allow him to prescribe when he was too old to carry his experience into practice? It may be urged with some reason, how can we expect to get pupils for technical instruction when so many of our artizans cannot even read and write? But thanks to the operation of the Factory and Education Acts, such a state of things is rapidly disappearing and giving place to a desire for instruction, which will eventually increase very much the applicants for higher Technical Education. I see no insurmountable difficulties in the way of utilizing almost every existing educational institution, from the primary schools up to the Universities. The child who is taught to write may at the same time get instructions in drawing. How early in life is this propensity shewn, and how much time at present employed in lessons in writing might with advantage be devoted to drawing: it would to many form an agreeable change, while in no manner lessening the chances of their proficiency in writing. Drawing is the universal language which speaks to all nations alike: it should be the companion of youth at every stage of his education, no progress in science being attainable without it. I would suggest that every district should establish Industrial Schools, adapted for its special branches of art and manufacture. Localities and individuals may originate, but it would be the duty of the Government to supplement them liberally. A parent considers the money spent upon his children's education as his most necessary investment, to confer upon them the means of fighting the battle of life. A good Government should, in like manner, devote a liberal proportion of its income to the proper training and education of the people, fitting them for the competition in which they are every day more and more engaged with all the world, and in which we must fall behind, if we do not go forward.

In 1871 we spent £10,901,144 for Poor Relief.
4,569,684 for Law and Justice.
1,755,489 for Education, Science, and Art,*

* Including—

British Museum,	£89,427
Education—Great Britain,	894,721
,, Ireland,	387,756
Learned Societies,	12,450
National Gallery,	90,747
Science and Art Department,	226,822
University of London,	9,577
,, Scotland,	18,000

The Endowed Schools and Hospital Commissioners, as most of you are aware, have issued circulars requesting answers to ten queries respecting Technical Instruction, shewing that the Government are taking an interest in the movement; and we may, I think, confidently hope that some decided national scheme may shortly be inaugurated to accomplish the object, the necessity of which so many earnest men have been urging for the last twenty years. The time for talking is past, action must now begin, and it is unnecessary to wait for a large system being introduced before setting to work in earnest.

I have to inform those who take an interest in this movement, that a collection of pamphlets treating upon the subject is being formed, and can be seen at the Glasgow Mechanics' Institution, Bath Street.

The VICE-PRESIDENT (Professor Young) congratulated the Society on the temperate character of the statement just read by Mr. Sandeman, who, while admitting the deficiencies of British work, did not ask us to believe in the imminent collapse of British manufactures.

MR. W. MONTGOMERIE NEILSON.—I do not propose to state any facts, or enter into arguments, to shew to you the very great importance of Technical Education in promoting the prosperity of our country.

I will only quote the words used by the Chancellor of the Exchequer, a few days ago, at the dinner of the Institution of Civil Engineers, in London :—"*We are competing against all the world; to stand still is impossible, we have to go on or go back.*"

The great question now before us appears to be,—What is Technical Education, and how is it to be taught? To this question I will confine myself in this short paper. Instead of using words of my own, I will quote from the report of the French Commissioners, appointed in 1868 to inquire into this question, which was translated and presented to both Houses of Parliament, by command of Her Majesty:—

"Technical Instruction comprises not only the exhibition of the applications of science to industry, and the practice of the processes employed in the arts, but also the demonstrations of elementary principles and the scientific knowledge connected with it."

The student, therefore, before he is capable of receiving Technical Instruction, must know something of science.

I will divide the education required by those who would carry on our industries in an intelligent and successful manner, into four distinct divisions of instruction:—

1. Primary Instruction;
2. Practical Art Instruction;
3. Scientific Instruction;
4. Technical Instruction.

In so far as we at present have to consider Primary Education, reading, writing, and arithmetic will include what is required. These we will happily now have efficiently taught under the new system of national schools, and also Scientific Education, as far as may be considered desirable, in order that every one may at least know something of the laws of nature, by which human life is sustained, and all things are regulated and maintained. In these schools, I trust, a standard of education will be obtained sufficiently high to qualify artizans for receiving Technical Instruction at the Technical Schools.

There must, however, always be science schools for those who may desire to prosecute scientific studies beyond what will be provided in the national schools, and who may not be able to go to our universities.

The kind of Scientific Instruction, or the extent to which it should be carried to enable the technical student to follow out his studies successfully, will depend upon what industry he is engaged in, and the position he proposes to occupy as an artizan, an assistant, an overseer, or a manager or master.

The Science Instruction which may be considered necessary for technical students can, with the exception of applied chemistry and mathematics, be given in the lecture-room; and even there, chemistry and mathematics to such an extent as will be generally required for ordinary Technical Education.

Practical art instruction, or the practice of arts and manufactures, is acquired by imitation and repetition in the workshop, without any necessary knowledge whatever of science, and often, unfortunately, with little or no primary education.

We now come to the fourth division of education—Technical Instruction—and the first and greatest difficulty will be to find suitable teachers. Primary education can be given without the aid of, or any reference to, practical art instruction, scientific instruction, or technical instruction. We can have no scientific instruction without primary instruction, and no technical instruction without practical art instruction. Consequently, no teacher can give technical instruction unless he himself has been instructed in practical art; he must not only be learned in science, but skilled in the art or manufacture in which he is to give instruction, otherwise practical students would have *no confidence whatever* in his teaching.

I am borne out in this decided statement by the Commissioners'

report already alluded to, where they state,—"Those studies intended for practical purposes cannot, therefore, be efficiently directed with all the befitting details of execution, with the explanation of processes and the necessary experiments, except by men who have themselves practised the arts whose principles and rules they have to explain, and who know how to speak the language of the ship-yard and the work-shop." And again, "It is, therefore, often on the very spot where the technical instruction is to be given, or in the workshop itself, that many of the professors ought to be chosen; and in general they must be sought among engineers, practical men, and manufacturers." The conclusion we arrive at is, that no purely science master or professor in a university can be a teacher of Technical Education. This, in my opinion, goes far to prove that it is necessary to have a Technical Institution or College, distinct from all other existing institutions.

The technical teacher, then, must be a man of more than ordinary intelligence and information, learned in science and practised in art, powerful in demonstration, and able to command the attention of men not habituated to study.

Technical workshops I consider altogether uncalled for; but there may be exceptions where instruction by an actual machine or process may be more advantageously and economically given than by models or drawings, such as weaving, dyeing, and some other industries. I look to the lecture-room as the great means of instruction for all departments of Technical Education, in conjunction with the experience gained in the workshop, illustrated by drawings, diagrams, models, specimens of material, and examples of manufacture. I may here remark, the most important branch of technical instruction is drawing; this aught to be the first thing taught to all students, in order that they may be able to understand clearly whatever is represented to them in plan, elevation, or section, or in diagrams, and be able themselves to put down by hand sketch a descriptive representation of whatever may be put before them, or convey to others intelligibly what they may have in their own minds or imaginations.

It has been asked, What is proposed to be taught in a technical school? I shall endeavour to give a simple answer to this question by taking a very few of our industries as examples. *First.* Engineering, or practical engineering generally; the subjects to be considered would be—strength of materials, how to be found and calculated; qualities of materials; weights of materials; simplest methods of finding them, &c. The proper application of different materials to various uses in machinery, stability, construction, motion, friction, lubrication, &c., &c., applied and worked out by static and dynamic laws, aided by an

ordinary knowledge of chemistry;—and more particularly would be considered Shipbuilding; forms of vessels, their capacities, strength, speed, details of construction for safety, durability, &c. Marine Engines; power, different forms, boilers, combustion, qualities and economies in fuel, &c., &c.; and so on, with all other varieties of steam engines, and machineries, and constructions.

Mining. Iron and coal pits, ventilation, draining, coal working machinery, winding and pumping apparatus, &c.

Iron-making. Furnaces, blast machinery, rolling-mill machinery, &c.

Textile Fabrics. Preparation of material, spinning, weaving, machinery employed, dyeing.

Glass-making. Materials used, perfection of materials, and art of fusing.

Pottery. Qualities of clay, kilns, glazing, colouring, fine arts as applied to, &c.

All these embracing the application of chemistry, geology, hydraulics, aëro-dynamics, practical mechanics, &c. It is admitted that in our factories and dwellings there might be an enormous economy effected in the quantity of fuel consumed. I believe if a well-supported Technical College had been established in Glasgow, we would now be reaping the benefit of it in a saving of a large amount of money in fuel.

Architecture and Building. Under these, the consideration of qualities of building material, foundations, forms and strength of structures, lighting, heating, ventilating, draining, gas and water supply, &c.

A course of technical instruction in such matters, from successful examples, by a practical teacher, would be most valuable to young architects, inspectors and clerks of works, and foremen, and is admitted by our most experienced architects to be much required.

I cannot take up your time by following this part of my subject farther, and will simply add one more example of what may be taught.

Commercial Science. We all know how much the success of many industries depends upon the management of the commercial department. Under this section would probably be taught commercial geography and history, technology of terms and forms used, accounts, theory of exchange, foreign exchange, measurement of goods, land and sea transport, productive consumpt, &c., &c., over the whole world.

I have been told all this can be found in books. But to this absurd objection the reply is, What is there taught that is not found in books? We have grammars and dictionaries in every language—books on

every science. Technical Education is, I hold, of all others, the instruction that cannot be taught from books alone, without visible examples and illustrations of practice and experience.

The great demand for labour of ordinary intelligence and sobriety, has made education now a matter of less necessity to the workman, in order to enable him to gain an easy livelihood. Young men of steady habits have for many years found no difficulty in obtaining employment in almost any department of industry, without being educated beyond what is acquired at any country school; and there are few trades in which apprentices remain a sufficient time under instruction to become *skilled* artizans. It is not, then, to be wondered at that employers complain of the difficulty experienced in finding men capable of taking the oversight and responsibility of conducting any of our great industries. That there are praiseworthy exceptions to this apathy for learning is happily true; but it is also true that there are no means within the reach of such men to assist them in acquiring the Technical Education they desire, and consequently they are discouraged by the labour in overcoming the difficulties of self-culture in applied science.

It has been frequently asserted, and no doubt with some reason, that there are ample opportunities for all classes to acquire scientific instruction; but what are the inducements held out, and what advantages do they see to be gained by the study of science? We cannot expect the ignorant and uneducated to study science for the love of it. But shew to them the use of science—its application to their daily avocations, how much it will aid them, and give them an interest and zest in their work, then you will throw a new light into their minds, and create a desire the very novelty of which will incite them to embrace whatever opportunities are offered to increase their knowledge.

It seems to me altogether erroneous to condemn the attempt to establish Technical Education, on the ground that the ignorance of science renders it most difficult to be taught. One of the grandest results to be derived from an efficient Technical College will be to create a taste and a desire for the study of science.

Then, as to the College, or rather, the building of the College. It should be in some situation convenient for meetings of directors, postal and telegraph communication, &c. The building itself, in the first instance, need not be a large one: containing a large lecture theatre, with ample arrangements for exhibiting and referring to large drawings, models, &c.; one smaller lecture-room, and three class-rooms, a library-room for scientific and technical books only; a large well

lighted hall or shed, lighted from the roof, where drawings can be be hung up, models, specimens, &c., arranged, all so as to be accessible to the students for study.

It is obviously evident that it would be absurd to expect students to come from all parts of Glasgow and neighbourhood to classes at this College. There are many young men of the wealthier class, the sons of foremen, managers, or employers, and others, who would attend the College during the day as regular students; but the great mass of students would be young men employed at the various establishments all over the city and neighbourhood, who could only attend classes in the evenings, after work hours; and to expect these, after a day's work, to walk several miles to a class, and be capable then of attending to their teacher's instructions would be unreasonable. To meet this difficulty, which is a most serious one, I propose the Technical College as a central institution, from which technical instruction would be sent out to all the outlying districts and suburbs of Glasgow. Whenever a suitable lecture-hall would be provided, by employers or by committees of the workmen themselves, and a guaranteed number of attendants taking tickets at a price fixed by the College, a teacher would be sent with the necessary drawings, models, &c., to give instruction in the department of technology required. There are, in almost every district and town around Glasgow, either Mechanics' or other Institutions, which could be put in communication with the Technical College, and thus provide instruction both for Glasgow and the whole of the West of Scotland.

I cannot see how Technical Education can be successfully carried out in any institution by fixed teachers or chairs. Any normal and uniform course of instruction, according to university curriculum, is altogether out of the question. Descriptive lectures by practical men who have studied science appears to me the only way in which a knowledge of the theory and science of arts in our multitude of industries, with their endless varieties of practical manipulation, can be given to the great mass of our industrial people. Under such a system I see no difficulty whatever in establishing a college of instruction, not certainly, fully equipped at the first outset, but by engaging such lecturers and teachers as can more immediately be found, taking up by degrees the most urgent subjects, and collecting illustrative apparatus and materials, the latter ultimately becoming a technical museum of industry.

I cannot conclude without noticing the great value of industrial museums in promoting a taste for art, and a desire for knowledge. The Science and Art Department of our Government, aware of this,

has done something in this direction; but it is much to be regretted that so little good has resulted from the great amount of money expended. There is the magnificent museum at Kensington, a most delightful place for those who have time and means at their disposal to go to visit it; but, for the industrial classes of either England or Scotland, because unapproachable, of no advantage. We have also a so-called Industrial Museum for Scotland, although very few of the hundred of thousands of those engaged in the arts and manufactures of Glasgow and the West of Scotland are aware of the existence of such a place, it being set down, not in the hive of industry in Scotland, but in the seat of learning, law, and the fine arts. Government will spend some £80,000 on this museum, which, although almost useless for the purposes of industry in Scotland, will form an interesting natural history museum, and an instructive place of amusement for the citizens of our modern Athens and their visitors.

My opinion is, that Government ought not to build and maintain industrial museums anywhere, but give grants-in-aid, in proportion to the private enterprise and liberality shewn by any district of industry where such an institution may be useful and desirable; the Government arranging to give from the Royal Museums, for limited times, articles to be exhibited; and the various museums over the different parts of the country exchanging with each other periodically, according to laws made and enforced by the Department of Science and Art.

I have had great difficulty in compressing into such a small space the above few remarks upon a subject so large, and I feel that I have done it much injustice. I have expressed my views on the somewhat popularly novel question of Technical Education, which I have formed from many years' consideration of the subject. Others may differ from me in some points, but I think all will agree with me in one opinion, that is, the time has come when we must do something for Technical Education; and I will be sorry if Glasgow takes a position in the rear of the march of intelligence.

MR. JAMES NAPIER said:—In reading over the reports of the meetings, held from time to time for the formation of a Technical College, I have felt at a loss to know what the members really wished, or what they really meant to teach in this college. At one time there is a proposal to fit up twenty looms, and at another time a dyehouse, as if it were to teach men trades. I do not think that this is required in Glasgow. At last meeting it was stated that it was not meant to teach men science. With such a confusion of ideas about the

object of the college, the delay in doing anything is not surprising, for a false step made at the commencement would take a long time to rectify This Society is no doubt aware that before any movement was made about a technical college, Mr. James Young had established and endowed a chair of Technical Chemistry in connection with the Andersonian University; but it is probably not aware that there is already subscribed a large sum for the establishment of a Technical Chair of Natural Philosophy including mechanics, naval architecture, &c. Observe, this is work done and doing, not talking and theorizing. The sum still required for the establishment of this second chair is only some £2,000 to £3,000. Would it not therefore be wise, as practical men, to set this agoing, and by these means to find out what is really required? for although these chairs may have a connection with the Andersonian, the admission of pupils for technical instruction is free to pupils of every institution in the country. We have had the experience of nearly two years in Technical Chemistry. and that has brought to light a desideratum we never dreamt of—a deficiency in the knowledge of the most elementary principles of the science. You are aware the trustees of the Young Chair of Technical Chemistry have had at their disposal nine bursaries of £50 each for three years. The only conditions for obtaining these are, *First*—That the applicants have not means of their own to pay for such a course of study; and, *Second*—That they know the elementary principles of the science of Chemistry. We did think that there would be no difficulty in getting our bursaries filled up. We have advertised again and again these two years; we have had upwards of forty applicants; we have confined the examination upon the principles to the answering of the most simple questions, and it is only this week that we have got the last two bursaries filled up. However, I think it but fair to state, respecting some of the applicants, that they could describe the mechanical process of an analysis, while they could not tell the nature of the reaction that took place there during these processes, shewing the defect to be more in the system of teaching what they had been under, in short, they were being taught the trade of Chemistry, but not its science. I cannot say whether the same defects will be found amongst those who wish technical instruction in mechanics and engineering; but I have been told by many large employers of such labour that there is a great deficiency in the knowledge of the ordinary principles of their trades. And I have heard some of those attending Mr. Laurence Hill's course of lectures say—"I wish I had got such information when I was an apprentice, I would have now been in a better position;" they had learned a trade

but not its science. I think it will thus be apparent that to spend £20,000 to £30,000 on establishing a technical college, without teaching the elements of the sciences, and teaching young men the mechanical processes of weaving or dyeing, would be a waste of power and money; it would be the play of *Hamlet* with the character of Hamlet omitted, for technical education cannot be given without the previous knowledge of the elementary principles.

I look forward to the time when the elements of science will be taught in common schools. And technical teaching will be a necessity in every high class school and college. We are falling into the rear of our German neighbours, let us therefore begin a friendly rivalry to regain the front rank.

MR. MAYER said he had devoted a good deal of attention to Industrial Museums, which, as Mr. Montgomerie Neilson had said, were indispensable to any educational scheme. He regretted that Glasgow had not accepted the offer, once in its power, of that Industrial Museum which is now in Edinburgh. In reply to Mr. Napier, he said that the teaching of Chemistry in schools was not only possible but actually carried on by himself as well as others. That these pupils, many of them well qualified, did not come forward to the examinations referred to, was due to this, that they could do better in commercial laboratories and other situations. He thought that the School Boards should take up the subject.

MR. JACOBY.—Being connected with the manufacturing department, I can assure you it is quite indispensable that a leading school should be erected in Glasgow. The manufacturers in Glasgow connected with the dress trade are constantly in such a critical condition that we see there is no other means of saving them than by the establishment of a Technical College. We, in Glasgow, have been for many years almost at the head of the worsted manufacture; but it seems now impossible for Glasgow manufacturers to keep head with the German manufacturers. One Glasgow house alone has this year introduced £1,800 worth of French goods. In respect to the cotton woven goods, Glasgow has been obliged to succumb in the coloured handkerchief trade. Large quantities come over from Switzerland. What is very strange is, that they are mostly composed of Turkey-red yarns, a manufacture of great importance to Glasgow, shewing that these manufacturers have cheaper modes of manufacturing that article. The manufacturers in Glasgow cannot explain it. Weaving, spinning, dyeing, &c., are taught in the leading schools in Germany,

and such schools must be erected in Glasgow. There is one other point—the difficulty of erecting such technical schools without at the same time raising the standard of primary education. We in Germany are not content with the three R's, but in every school have Natural History, Geography, General History, Chemistry, and the elements of Physics taught. I was greatly surprised when in my native place lately—a place of only 2,500 inhabitants—to see that English and French were universally taught in the public schools. Naturally such schools are great feeders for technical schools. Another fact, most middle-class people require their children to go to higher class schools. The great links between the primary schools and technical schools on the Continent are the middle schools. Very few children of middle class parents leave the schools before fifteen or sixteen. With respect to commercial people, it is quite unusual to receive any one into a commercial house who is not able to speak several languages. They would not come forward as candidates without receiving much higher education than here. Perhaps the cure here would be to enforce the same higher standard before receiving them.

Mr. Neilson.—As to Mr. Napier's recommendation that we should wait and see what our requirements are before we go much further, I would just simply reply in the same line of argument, that we ourselves know our wants. As an engineer, I know what I want; as a shipbuilder, I know what I want; as an architect, I know what I want. Gentlemen connected with various industries have expressed their wants, and I don't think it is right in Mr. Napier to try to oppose them. The industries themselves have come forward and demanded these things. A Committee has been formed of gentlemen connected with them to try and get their wants supplied. As to our friend's remarks about primary education, certainly it must be brought up to a higher mark. But we have institutions to do that. There ought to be a supply of scientific teaching from the University that would do for three Glasgows.

Mr. Napier.—I was thirteen years a dyer, and I know that we could learn dyeing, and be first-class dyers, yet know nothing about its principles; we could learn it as a mechanical process.

Dr. Bryce.—I think the case has been thoroughly made out, even before the papers read to-night; and the only question is, "How can it be carried out in Glasgow?" I confess to have a good deal of

sympathy with what Mr. Napier has said, with regard to making use of the institutions that have long existed in Glasgow. Professor Anderson, in founding his University a century ago, had the benefit of the working-classes in his eye, and we all know that the Andersonian has been the beginning of scientific light and thought to great numbers of the working-classes in Glasgow. Then there is the Mechanics' Institution. You know the history of that Institution, and it is now the only one that exists in any degree of efficiency in this country. I should like to see the technical college connected with some of these institutions. I regret very much the strong step Mr. Neilson has taken in separating himself from both institutions. We are much indebted to Mr. Neilson for the sacrifices he has made, and the amount of thought he has given to this subject; but I would strongly urge upon him to try and carry out his original idea, and connect the proper technical college with one of these two institutions. It would be a saving of money, and an expression of gratitude to the founders of these institutions, to say how useful we thought they had been. I think Mr. Neilson's idea a good one of sending out teachers to different districts. There is another subject in which Professor Young and myself have taken a great deal of interest, and that is the mining school. I think it is a disgrace to this country that while 150 years will exhaust the whole of our coal and ironstone, we have never yet had a mining school. The men who have amassed large fortunes, and who, two or three of them, could have established a school of that kind, ought to do so. I was much interested in the speech of Mr. Jacoby, for it explains to me how the young men in Germany can take so much time in their education. He has given a very practical hint to our merchants here. I find the greatest difficulty in getting situations for the higher educated young men. I take a boy, and tell what he can do. They say, "O, he is too good for us; give us a boy that is good at sums."

PROFESSOR YOUNG said:—He regretted that antagonism had been suggested between educational establishments, and that names had been introduced most needlessly and, he suspected, without authority, into the discussion. He went on:—We have the three institutions —the Gilmorehill, the Andersonian, and the Mechanics' Institution. I know that Mr. Montgomerie Neilson is not desirous of divorcing the college from the Mechanics' Institution. As to the Andersonian, there is a difference of opinion on the point. But I think it would be a matter of regret if the University should be connected with the work that falls properly within the domain of the technical

college. The Andersonian and the Mechanics' Institution have a claim to carry on and extend the work they have begun. How far it would be practicable to adapt the one or the other to the purpose it is not for me to say. Mr. Napier, he pointed out, had answered his own statements in part, and Mr. Mayer had completed the reply. The case was exactly that of the Indian Engineering College at Couper's Hill. Engineers had not come forward for the Indian service who could do better elsewhere, but this did not mean that Engineering was not properly taught in Britain.

He regretted that manufacturers in one department, or representatives of an institution, should thwart the schemes of others who prove that their department is in need of improvement, or who shew that a new institution is needed for some particular purpose not served by existing ones, or for which they could not be adapted.

A good deal has been said of the apathy of the working-man to science, but as division of labour increases, multiplication of machinery is inevitable, and these new machines tend to reduce the position of the working-man to that of a "factor" in a machine. The more this is done —and it must go on—the more will the working man be reduced to apathy in regard to science. The scientific instruction in these institutions would to some extent diminish though not remove grievances under which we are all more or less clamorous. The migratory instruction to which Mr. Neilson has referred is likely to become the most prominent duty of the Universities in Scotland, England, and Ireland. I propounded a scheme in the "Mail" six years ago, that the instructors in mining should migrate to the places where there were the greatest number of pits, and should there give instruction. The University of St. Andrews has set a noble example in the way it has held on to its point, and succeeded at last in obtaining efficient hearty support from the merchants of Dundee, establishing in Dundee a branch of the University, and thereby extending the benefits of the University. I hope the day is not far distant when, by combination among Scottish Universities, we shall have affiliated schools in towns such as Greenock, Stirling, Perth, or other important centres throughout the country, in which instruction may be given in particular branches, the pupils of such affiliated schools coming afterwards into the Universities or not as might be the case. I fear it will be many years before we can expect this. In the meantime we should give all possible encouragement to the improvement of education in whatever branch; and, after we have got one started, proceed to what is the next most prominent one and start that. In this way only can we hope to do any good.

XXII.—*The Bank Acts, and the Rate of Discount.*

By MR. STEPHEN MASON.

Read before the Sanitary and Social Economy Section of the Philosophical Society, March 3, 1873.

THE Bank Act of 1845, for regulating the issue of bank-notes payable on demand, and the monopoly in banking which it has created in Scotland, has of late attracted a considerable amount of public attention. There is no doubt whatever that the question must be dealt with by the Legislature at no distant date. We have a special grievance to complain of in Scotland, as compared with England, from the fact of having enjoyed the inestimable privilege of a perfectly free system previous to 1845—a system with which the Scottish people were perfectly satisfied, and under which they have made astonishing progress in manufacturing and commercial greatness. Yet, although it was proved to be sound in theory and successful in practice, we had to give way to English notions; and the perfect was linked to the imperfect, when the proper and legitimate course should have been precisely the reverse. Had Sir Robert Peel assimilated the English systems—all of which were faulty, and never were free—to that of the Scotch, our agitation to set things right would not now have been required. The question must therefore be looked at from an imperial point of view; and before the Act of 1845 is repealed, that of 1844 must be swept away. I purpose, then, to treat the question in its widest sense, and to consider it both in its theoretical and historical aspects. Theoretically, let me say, that in a rude state of society commodities or property are exchanged by a system of barter. The second stage in civilization produces a circulating medium usually, although not always, composed of the precious metals. These, owing to their uniform steadiness of value have presented themselves to all generations, both in ancient and modern ages of the world, as the most suitable, and are in use now in all parts of the civilized world. Gold being the British standard or measure of value, all transactions are measured by and resolvable into the pound sterling as the legal coin of the realm. The sovereign, which is stamped by the Master of the Royal Mint to shew that it

contains a given quantity of gold of a given purity and fineness, is the only legal tender in Scotland above forty shillings, silver being legal tender to that amount. In England, Bank of England notes are legal tender outside the walls of that establishment, which may fitly be characterized as a legal blunder. The functions of the State in the creation or coining of money are simply of a mechanical nature. Many people suppose that the value or price of gold is fixed in this country. This is a mistake; it is not so. All that is fixed is the weight and purity of the sovereign. So many ounces of gold are carried to the Mint, and a given number of sovereigns must, according to law, be returned to its owner, these coins bearing the impress of the reigning monarch, to shew that they are genuine, and when tendered in payment of a debt they cannot be refused. The public both create and supply the demand for this money. If a scarcity of sovereigns exists, the Bank of England, or some of the other banks, feel it immediately through their customers, and more bullion is coined into sovereigns to supply the demand. Neither the Government nor the Master of the Mint buy or sell the gold. The prerogative of the State is thus reduced to a mere process of stamping, which is a very simple prerogative indeed, and altogether different from what would be absolutely necessary were the same principle applied to the issuing of legal-tender paper money. In the one case, it is safe and sound, and for the public advantage; in the other, it would be utterly unsound, and fraught with manifold evils to the community. In point of fact, it is too absurd to be seriously considered. Let us suppose the principle of dealing with paper money applied in the same way as the State deals with metallic money, and what follows? The State has fixed very properly what should be the weight and purity of the sovereign, and authorized all the gold brought to the Mint to be fashioned into the legal coin of the realm. But would it be a right and proper use of the prerogative of the Crown, or a wise and statesmanlike act, to make it the prerogative of the Crown to authorize the issue of legal-tender notes either to individuals or to banking institutions, not excepting the Bank of England? Verily, we should soon have notes in abundance, and not worth, in purchasing power, the paper upon which the stamp had been impressed. The issue of legal-tender notes by the State is but the other side of the same thing; the only difference being, that the Government is supposed to take the place of the public. This is no new notion. It has been tried in both hemispheres of the globe, only in periods of dire necessity, and in every instance with disastrous results and great depreciation of the purchasing power of the notes.

In France the over-issue of such notes, known under the name of assignats, was such, that in June, 1793, one franc in silver was worth three francs in paper; in August of the same year it was worth six. The State tried to meet the difficulty by fixing a maximum of prices; but no one could compel producers and dealers to sell at a loss, so that all business became disorganized; at last the value of assignats was reduced almost to nothing: 500 francs in paper would scarcely purchase a cup of coffee.

In the United States of America, during the recent civil war, when the Government was in great straits for money, it had recourse to the same plan of paying its debts by issuing paper money, commonly known as "Greenbacks." The excess of issue was such, that at one time one dollar in silver was worth more than two in paper; and even now, March, 1873, the State notes are still circulating at a discount of from 10 to 14 per cent.

The only way by which Government might hope to manage effectively a State issue of paper money without depreciation, would be to make it payable on demand—a condition which, we agree in holding with Adam Smith, is absolutely required to prevent over-issues and depreciation. If the State undertakes this duty, then it must become a dealer in gold bullion, and hold at all times a stock sufficient to sustain the currency. Not only so, but it would require to open offices throughout the country for the payment of these notes. The profit in that case would vanish, and, in point of fact, render the scheme worthless, and all but impracticable. We feel sure that Mr. Lowe, on the part of the existing Government, would be one of the first to repudiate the idea of adding to the duties of his office by becoming a dealer in bullion. But how he could manage to get rid of this necessity, if the conditions he laid down in his letter to the Glasgow Chamber of Commerce were carried out, does not readily appear. He stated, in the letter referred to, "that the issue of notes payable on demand is the creation of money, and that the creation of money is the business of the State." He stated, further, that a mixed currency should comply with the three following conditions:—"*First*, The paper must be convertible into gold on demand. *Second*, Sufficient security must be held by the issuers to secure the payment of the notes. *Third*, The mixed currency must be at all times exactly of the same amount as a purely metallic currency would be." If, as Mr. Lowe says, the paper must be convertible into gold on demand, it necessarily follows that Government must hold at all times a sufficient amount of gold for this purpose. If not, how does he propose to pay the notes on demand? There would be less difficulty with the second condition

in providing proper security. But the third condition never has been accomplished by any State, and could not be successfully carried out—unless, indeed, Government became the bankers of the country, which we are not prepared for, nor is it to be desired. These three conditions are not new. They were fully recognized in Scotland more than a century and a-half since, and successfully carried into practice by a system of banking and currency absolutely free.

The premises upon which the Chancellor builds his theory, that the issue of notes payable on demand is the creation of money, and that the creation of such money is the business of the State, are, in my opinion, quite unsound. No doubt, notes payable on demand become in a certain sense money, if the public use them as a circulating medium; but there is an essential difference between a bank-note and a sovereign. The one is merely a bit of printed paper, intrinsically worthless—a promise to pay a sovereign; whilst the other is the thing promised. Does any one believe that the promise to do a certain thing is the same as an accomplished fact? Our able junior representative for the City, Mr. Anderson, would appear to have fallen into Mr. Lowe's way of thinking about money. He is reported to have said at a recent meeting in the City Hall,—"I confess I fail to appreciate the intellectual obscurity of a merchant who cannot see the difference between a bank-note and a cheque." He goes on to say,—"The difference is, that a bank-note passes from hand to hand without endorsement, but a cheque has the liability of the party passing it by endorsation." The latter, according to this theory, is therefore an instrument of credit, but the former transcendently and essentially different. We confess that we fail to appreciate Mr. Anderson's intellectual acuteness in this particular. Why, it is only when the cheque is made payable to order that it requires to be endorsed. If it is made payable to the bearer, it may pass through fifty or five hundred hands without endorsement, provided people are willing to take it, and it is therefore precisely and in all respects the same as a bank-note—both being purely and simply instruments of credit. Mr. Anderson further said, "that it appeared to him something very nearly approaching insanity, to suggest that anybody ought to have the power of issuing bank-notes who pleased, and that the whole remedy of the public is not to take them unless it likes." We are surprised it never occurred to Mr. Anderson that he, as an individual, or anybody else, is quite at liberty to issue, without let or hindrance, as many cheques or, in other words, as many promissory notes as the public choose to accept, and he or anybody else is willing to issue. It was a false idea that bank-notes

payable on demand could be over-issued, which led to the passing of these Acts; but it has never yet been proved that such was the case. It is quite clear that unless the notes command the value they bear upon the face of them in money or money's worth, they will not circulate. In point of fact, a bank without some foundation for its credit would not get its notes into circulation. We agree with Adam Smith when he says,—"If bank-notes are made payable on demand, they may be left in all other respects perfectly free." Capital is one thing, currency quite another. If the currency in use be wholly metallic, it must be extremely inconvenient, and only suitable for a country very little advanced beyond the stage of barbarism. You have only to imagine the gigantic transactions which take place every day in our own city, settled or paid for in hard cash—to consider the impracticability of a purely metallic currency, besides the loss which it would cause of labour, tear and wear of coin, and the capital which would be absorbed.

All the functions which a metallic currency performs are much more expeditiously, economically, and safely performed by bank-notes, cheques, and other instruments of credit.

The advantages of this method for economizing the currency were well understood and acted on towards the end of the seventeenth century by the founders of the Banks of England and Scotland; and in order to shew the origin and use of bank-notes, we shall now sketch briefly the history of banking, and the issue of bank-notes in both countries, down to the passing of the Bank Acts of 1844 and 1845.

The History of Banking in England.

During the reign of the Prince of Orange, not long after the beginning of the national debt, the Government, in order to carry on the wars which that extraordinary prince thought it necessary to wage in defence of the honour and power of England, required money, which supplied them, as it does still, the chief sinews of war. Schemes innumerable, and of the most absurd nature, were ushered into existence. In the extremity, an obscure but clever Scotchman, by name William Paterson, prepared and submitted a plan for a National Bank which found acceptance in Parliament. A charter was granted to a company, who founded the Bank of England in 1694, empowered with exclusive privileges, in consideration of which they agreed to furnish £1,200,000 to Government as a loan at 8 per cent. per annum. The year 1708 is memorable in the history of banking for the worst Act ever passed in connection with this great interest. It was enacted then that, during the existence of the

Corporation of the Bank of England, it should not be lawful for any body politic, erected or to be erected, other than the Company of the Bank of England, to embrace more than *six partners*. No better plan could have been contrived to render all her competitors weak; and, as might have been expected, it was the source of the greater number of banking disasters in England for upwards of a century. In 1793 the Bank first commenced to issue five pound notes; but 1797 was the most memorable year in the history of the Bank of England.

Pitt had just entered upon one of the most tremendous struggles this country was ever engaged in, against the First Napoleon, and was making frequent calls upon the Bank for loans to the Government. The directors remonstrated with the powerful Minister, but did so in vain. At last, owing to their diminishing resources, they were compelled to refuse further aid. Pitt saw that either he must stop the war or stop the Bank. He resolved upon the latter course; and by an Order in Council, dated Sunday, the 26th of February, 1797, the Bank of England was commanded to stop cash payments. Great alarm was felt at first, but it soon died away, and the public accommodated themselves to the new order of things, without calculating upon the day of reckoning which awaited them. The Bank daily issued promises to pay gold, which were met, when presented, by another promise to pay of the same description. Of course, these documents were only accepted in the country, where people had no redress, and were useless for making payments abroad. A large efflux of gold consequently ensued, and a great depreciation of the purchasing power of bank-notes took place, varying from 3 to 25 per cent., till the resumption of cash payments in 1819. In 1810 things were in a bad way. Rents had risen largely, and the prices of commodities were extreme, caused to a great extent by the redundancy of the currency. At this time a Committee of the House of Commons was appointed to inquire into the comparative high price of bullion and the state of the exchanges. An able report was furnished by Mr. Francis Horner, who very sensibly recommended the resumption of cash payments in two years; but the House would not listen to this proposal, and do not seem to have understood the subject, as they agreed, by a large majority, to a resolution, proposed by Mr. Vansittart, to the effect—"that the Bank of England promissory notes had hitherto been, and were at that time, held to be in the public estimation equivalent to the legal coin of the realm." The absolute nonsense of this resolution only needed another paragraph, asserting that two and two make five, to render it complete. Common sense and arithmetic would then have

been equally outraged. In 1814, no fewer than 240 Banks failed throughout England, causing much suffering and distress. It was said in Parliament that the national miseries at this time had risen to a point wholly without precedent since the Norman Conquest. The resumption of cash payments was completed in 1821.

Confidence being restored, trade and commerce moved on with comparative smoothness till 1824, when a speculation mania floated schemes of the most wild and ridiculous nature. In the following year a fearful crash occurred, the Bank of England being nearly exhausted of its stock of bullion. The issue of one pound notes had been suppressed from 1821; but it so happened that an old box containing a quantity of these notes was discovered in one of the cellars of the Bank, and forthwith issued, which is generally supposed to have saved the Bank from suspending cash payments. In less than six weeks seventy of these six partners' Banks failed, entailing much loss and suffering on the community. They had the privilege of issuing one pound notes, which, curiously enough, the House of Commons thought was the cause of all the mischief. In 1826 an Act was passed suppressing the issue of one pound notes in England; and at the same time it was proposed to extend the provisions of this Act to Scotland. The Scottish people resisted this attempt to interfere with the issue of Bank-notes on this side of the Tweed. It called forth those famous letters of *Malachi Malagrowther*, of historical fame, which had almost a magical influence, and Scotland was allowed to retain her cherished one pound notes. In 1837 a temporary embarrassment occurred in monetary affairs, which was followed by a crisis in 1839, occasioned by a series of bad harvests. Between this period and 1844 there were eighty-two failures of Banks, of which only twenty-nine were Banks of Issue.

Of the whole number forty-six paid no dividend, and twenty-four paid dividends under 10s. per pound. These statistics were furnished by Sir Robert Peel, and he appealed to the House of Commons, with regard to them, in these words:—"Surely the conclusion we must draw from the details to which I have referred, both with respect to the failures of banks and the loss and suffering entailed upon their creditors, is in favour of legislative interference." The answer to this question ought certainly to have been in the affirmative, but the interference was altogether in a wrong direction. The issue of bank-notes was again blamed for producing this lamentable state of things; while, strangely enough, the fact was that only twenty-nine of the eighty-two failures referred to were Banks of Issue. The true policy was clearly to allow and facilitate the forma-

tion of banks with the same privileges, upon the secure foundation of the joint-stock principle, such as had been so successfully established in Scotland, and to have had less anxiety about the convertibility of the notes.

The crisis of 1825 was attributed to the issue of one pound notes by the country banks, they were in consequence suppressed in England and Wales. Notwithstanding this, the crisis of 1837 and 1839 came and exposed the shallow reasoning of these superficial thinkers. A new theory was then propounded by Mr. Jones Lloyd, now Lord Overstone, which was adopted by Sir Robert Peel,—viz., that while the Bank of England contracted her issues, or, in other words, did less business when her resources were reduced by the withdrawing of deposits during an adverse foreign exchange, the country banks increased their issues and did more business—which was a most natural thing for them to do—and by getting their deposits employed, must have proved, not only beneficial to the banks, but also to the country. This, in my opinion, was one of the best reasons which could have been urged in favour of freedom, instead of which, these currency doctors considered it a most dangerous symptom, and forthwith proceeded by restrictive measures to seek its suppression.

The History of Banking in Scotland.

The history of banking in Scotland presents very striking and different results to that of England. Lord Macaulay held that her schools and banks had transformed Scotland. We believe he was perfectly justified in holding this opinion. The Bank of Scotland was founded in 1695 : the estimated amount of coined money in the kingdom was then only £100,000. The Bank was established by Act of Parliament, and obtained a Royal Charter which secured to it the exclusive privilege of banking in Scotland for twenty-one years. The capital of the bank was £100,000. Its first issue of notes was for five pounds and upwards. Not till the year 1704 were one pound notes issued for the first time in Scotland. The success of the bank was somewhat chequered at first, but, after getting rid of the taint of monopoly, it made rapid progress and established itself securely although surrounded with many difficulties and much opposition. The Royal Bank of Scotland came next in order of time, being founded in 1727, which was followed by the British Linen Company in 1746. A considerable number of private banks were begun during the latter half of the eighteenth century, but the joint-stock principle found most favour in Scotland; the Commercial Bank being established in the year 1810, to supply the growing wants of the country,

which was followed rapidly by others previous to 1845. All the private banks had disappeared long before any legislation was thought of for regulating the issue of bank-notes. It is a great and telling fact that no Joint-Stock Bank in Scotland has failed under a system of freedom, without paying every penny of its creditors in full.

The year 1797 witnessed the suspension of cash payments in England, and the subsequent depreciation of Bank of England notes; and whilst these were circulating south of the Tweed for nearly twenty years at a depreciated value, Scotch bank notes were circulating all through that trying period at their full value. This is, perhaps, the best proof which could be adduced in favour of a free system of issue, and as condemnatory of State interference with the issue of notes payable on demand. A practical illustration is thus afforded of both systems existing alongside of each other in our own country, the result being triumphantly in favour of freedom! For nearly 130 years previous to 1845, Scotland was entirely free from legislative interference with regard to banking and the issue of bank-notes, and we venture to think that no country in the world could shew better management or a more perfect system.

It has called forth the admiration of distinguished and eminent men, both at home and abroad. In a letter which I received from the distinguished French political economist, M. Michel Chevalier, in 1866, he says:—"You are in Scotland a most enlightened people. The celebrated verses of Voltaire, which he applied to the empire of the great Catherine, and which was a mere flattery to this sovereign, may be, with much more truth, applied to your native land—

"C'est du nord aujourd'hui nous
Vient la lumière.'"

The French Government, under Napoleon the Third, instituted a most searching enquiry into the question of banking and currency, having collected a vast amount of information from nearly every country in Europe. Scotland and the Scottish system fills a very prominent position in these interesting volumes which, it is to be hoped, will be of use to France some day.

The Bank Acts of 1844 *and* 1845.

The question as to the issue of promissory notes, payable on demand, was thought to be, in 1844, one requiring legislative interference, and led to the passing of these Acts. The argument may be briefly stated thus:—

First. That the convertibility of the notes is not secured where they are left perfectly free, although made payable on demand.

Second. That an uncontrolled issue of bank-notes would result in "over issues," and, consequently, the currency will not vary as a metallic one would do.

Third. That the paper currency ought to be regulated with a regard to the foreign exchanges, as indicated by an influx or efflux of gold to and from the Bank of England, and this, in turn, should regulate the rate of discount.

We have already disposed of the first and second of these propositions in our remarks upon currency. We shall therefore proceed to examine the third in connection with the effects of these Acts since 1844. The usury laws were only virtually repealed in 1833. Down to that period it was not lawful to charge more than 5 per cent. for capital lent. During the brief period which elapsed between that year and the passing of the Bank Charter Act, in 1844, a period of eleven years—when it may be said the Bank of England was not artificially controlled—there were only five changes in the rate of discount, the fluctuation being from 4 to 5 per cent., with the exception of a few months during the crisis of 1839, when it touched 6 per cent. The Act of 1844 had been but a few months in operation when the Bank Directors, for the first time in her history, lowered the rate from 4 per cent. to 2½ per cent., and it remained stationary for more than twelve months at this low rate. Merchants do not like an exorbitant rate of discount, but it is a grave question whether a low rate is not productive of more harm than one exceptionally high. We are disposed to believe that the cause of the crisis of 1847 may be easily traced to the cheapness of loanable capital which prevailed at this period, and that the Act introduced a new principle by which the Directors were guided in regulating the rate of discount. Formerly the Bank of England had no limit imposed upon her as to the issue of bank-notes, neither had she injunctions from the Legislature to issue notes against all the gold that might be, from whatever cause, deposited in her vaults; and that these notes, or rather an equal amount of notes, should be destroyed when the gold was withdrawn, thus producing a scarcity or a glut of notes alternately. The ruling idea which led to this unfortunate blunder, arose from a notion that the rate of discount and the foreign exchanges should be governed by the currency, which is reversing the natural order of things, and is like putting the cart before the horse,—the servant above his master. It would be quite as sensible to regulate the price of flour by increasing or decreasing the number of grinding stones in the country according to the influx or efflux of gold; or to raise the price of bread by restricting the number of bakers by act of Parlia-

ment. Currency, being merely an instrument for the convenience of capital, ought to be at all times sufficient for its wants, and should not be made redundant by a forced issue, although a large quantity of gold happens, by accident or some other cause, to be sent into the Bank of England. Those who watch the rate of discount, and are interested in its movements, look at once for the published returns of the Bank of England, and the reserve is the turning point upon which the matter rests. Now, if you examine the figures published weekly, you will observe that the reserve is acted upon in two different ways,—by the depositors of the bank withdrawing gold, which compels the Directors to cancel or destroy notes to a like amount; or by an absorption of notes when trade is active; or during a panic when they are hoarded, such as occurred at the time of Overend, Gurney, & Co's failure. If it so happens that these two causes are concurrent, the effect is an alarming advance in the rate of discount—it may be to 7, 8, or 10 per cent. without having the desired effect, the suspension of the Act being the only remedy—such as was witnessed in 1847, 1857, and in 1866. It is the limit imposed upon the bank which creates the excessive demand and the hoarding of the notes, and this greatly aggravates and intensifies the crisis when it does come. When merchants and bankers observe the reserve of notes in the bank falling, and discount rising rapidly—it may be an advance of 3 per cent. in nearly as many days—an ugly rush and scramble is made to get hold of the notes which are not returned in the usual way, but locked up for future use, not being set free until the inevitable Government letter makes its appearance, all danger of a suspension of the bank being then averted by setting aside the limit. It has only once, I think, been reached, clearly proving that its existence produces the supposed danger, *and that it should not be there.*

In 1847 the rate of discount was raised, by one convulsive leap, from 5½ to 8 per cent., but this had no effect, and relief was at once obtained by a suspension of the law. In 1857 it was advanced, between 8th October and 9th November, from 6 to 10 per cent., but yet again suspension was the remedy. In 1866 the rate was raised, between May 3d and 12th, from 7 to 10 per cent., being kept at that highly destructive point till 16th August, a period of three months after the Government letter authorizing a suspension of the law had been issued;—one of the conditions of the letter being to the effect, if the rate was reduced below 10 per cent., the letter was held to be cancelled. The bank being afraid to part with the letter could not reduce the rate, and the public, like a patient

beast of burden, had to bear the yoke. The Act of 1844 drew after it that of 1845, which you are all aware is applicable to Scotland. It has created a practical monopoly of banking on this side of the Tweed. But its operation upon the gold in the Bank of England, and consequently on the reserve and the rate of discount, is at critical periods of the money market very injurious and quite unnecessary. The Act allowed the existing banks to retain their then right of issue of notes payable on demand, and forbade the formation of any new banks with the power to issue notes. It is this latter condition which has prevented the establishment of any new banks in Scotland since 1845. A bank without an issue of notes would be regarded by the bulk of the Scottish people as no bank at all. Besides, the prejudice which would thus exist against it, the disadvantage at which it would compete by the loss of profit on its circulation, would be much greater than at first sight appears. It is well known that the law, although maintained by the existing banks in the letter, is daily broken in the spirit, the circulation being sometimes nearly double during the week what appears in the Government returns—the banks having by their admirable system of exchange with one another, reduced the excess by the time the return falls to be made. But at Whitsunday and Martinmas terms, when the payments of rents and wages increase the circulation for a few weeks, this cannot be accomplished in the same way, and the banks are compelled to send, in the end of April and October, to London, for gold, against which the law allows them to issue notes. Now, what is the use of this gold were a run to take place upon the banks? A few depositors could exhaust the whole stock before the noteholders knew anything of what was going on. It was the depositors withdrawing their deposits, not the noteholders, which compelled the Western Bank to close its doors. The gold is wanted in London, but is no use whatever in Scotland, going back to London in most instances in a month thereafter, without having been opened out of the packages. Sir Robert Peel thus created a real pressure to get rid of an imaginary one, and, as already stated, the reserve of the Bank of England being affected by the operation, the usual result is frequently an addition of 1 per cent. to the rate of discount, in consequence of the Scotch demand. It will be readily granted that supply and demand ought to regulate the rate of discount, which is but the price of loanable capital. In consequence however of the monopoly, and the restrictions on the currency, which is the instrument by which the loanable capital is transferred from hand to hand, this is now too often practically set aside. The banks

ascertain the supply of loanable capital at their command by the amount of their deposits—they act as reservoirs, deriving their supplies through those numerous branches spread all over the country like so many feeders. The deposits held by the Scotch Banks now amount to £70,000,000 sterling, having increased not less than £4,000,000 during the year 1872. Five of the London Joint-Stock Banks have amongst them close upon £100,000,000 of deposits, the London and Westminster exceeding very considerably those of the Bank of England. But for the monopoly of issue conferred upon the latter institution by the State, her lending power would not now equal that of the London and Westminster, and but for the tremendous power vested in the Bank of England through the exclusive privilege of the issue of legal tender notes, and other privileges, they would be more on a par with regard to influence. The Bank of England may justly be called a great engine of State for the purpose of working out the currency principle of metallic and paper variations, and it is without doubt the chief cause of the violent fluctuations and vexatious number of changes now experienced in the rate of discount. Since 1844, we have had fluctuations in the rate of discount, frequent, extreme, and sudden; and it requires no further argument or illustration to shew that a system of banking and currency which was suitable for 1844 must be quite inadequate for 1873. A law which cannot be shewn to do any good, and is accused of doing a vast amount of harm, having been suspended three times, must surely be a bad law, and ought therefore to be repealed.

The Remedy.

The practical question remains to be answered, viz., What shall we do to remedy the evils complained of? It would be a difficult task to discuss all the plans which have been from time to time suggested. We shall simply submit for your consideration what appears necessary, and what has been already sanctioned by reason and experience. One thing is inevitable;—the monopoly in connection with our banking institutions must go the way of all monopolies of the past, not only in Scotland, but also in England and Ireland. In order to accomplish this desirable object, two courses are open for us to follow: either to withdraw all existing exclusive privileges from the Bank of England, and all other banks in the country, or to confer equal privileges on all who choose to comply with the same conditions. The latter course is clearly impracticable, seeing that the Bank of

England has special privileges which cannot be extended to others, such as the monopoly of issue for the whole of London, and for a radius of 65 miles around the metropolis. We are thus thrown back upon the other course, and when all special privileges are withdrawn you arrive at a system absolutely free. Free trade in banking and the issue of notes payable on demand, would be good for the banks and safe for the people. But although free trade is the true and proper solution, it is quite possible a less perfect plan may be adopted. That which is perhaps the most popular is a proposal to allow all banks freedom to issue notes against Government securities, so that, in the event of failure, the Government would pay the noteholders. This plan would appear to commend itself to many people, both north and south of the Tweed. It has always appeared to me objectionable, because you would most surely foster weak banks, and entrap the people to do business with them by the fancied security which the notes would give to the public. These being secured, would be readily accepted and pass into circulation, and deposits would be encouraged. Now, the deposits are by far the most important matter of the two. The circulation for Scotland is perhaps a little over £5,000,000, whilst the deposits amount to the magnificent sum of £70,000,000. If the banks are considered good enough for the latter amount, surely they may be trusted, without Government interference, to pay the very much smaller one. Should this plan be adopted, other precautionary measures would be necessary. A third plan, and, to my mind, one that is preferable to the second, would be to compel all banks to retain a given portion of their liabilities in gold, say a tenth part, or whatever proportion a number of competent bankers would concur in considering a safe and proper reserve. In conclusion, we trust the discussion may help to form public opinion, and lead to a wise and just solution of a question confessedly surrounded by many difficulties. The law of supply and demand, if not artificially controlled, usually acts most beneficially for all. The great principle of freedom has been applied to nearly all branches of industry, with results of which its advocates never dreamed. There is no reason why it may not be applied, with equal success, to one of the most important interests of the country.

XXIII.—*On the Decomposition of Sulphate of Potash by Nitrate of Soda.* *By* MR. WILLIAM HENDERSON.

Read before the Chemical Section of the Philosophical Society, January 13, 1873.

ABOUT twelve years ago, when making some trials to produce saltpetre from Canadian potashes and nitrate of soda, with a few ounces of material, I came to the conclusion—although perhaps a wrong one—that nitrate of potash and nitrate of soda had the power of increasing the solubility of some salts, such as those that are found mixed with them in a crude state—the sulphates and chlorides of the alkali metals. Muriate of potash being high in price at the time (£20 to £22 per ton), and sulphate low (about £20), there was some inducement to use sulphate in the making of saltpetre, if it could be accomplished. The slight solubility of sulphate of potash, compared with that of the other salts that were being used, was much against the idea; but the following note was made at the time:—"Nitre seems to have the power of increasing the solubility of other salts: would nitrate of soda dissolve an increased quantity of sulphate of potash? if so, nitrate of potash might crystallize from the solution," or decomposition might take place under pressure. In boiling down a solution of kelp the various salts are crystallized, or fished out, apparently according to their degree of solubility, sulphate of potash being thrown down first, then common salt and carbonate of soda, then chloride of potassium. Now, with such an easily crystallized salt as nitrate of potash, the reverse of this might happen. The nitrate of soda might retain the sulphate of potash in solution, and the nitrate of potash, being a still more soluble salt than nitrate of soda, may induce the decomposition by its greater solubility and readiness to crystallize, even in hot solution.

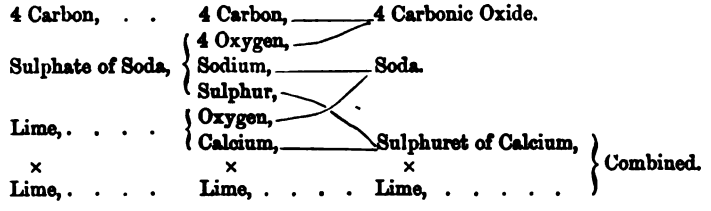
On proceeding with the making of saltpetre, although there was 12 to 15 per cent. of sulphate of potash in the potash, or 6 to 8 per cent. in the whole material used, there seemed no indication of its collecting in any quantity in the liquors or residual salts; it was supposed to be lost in the latter, and although found in these salts it was not in sufficient quantity to account for the whole amount. It was then suspected that it was decomposed by the nitrate of soda; and, to prove this, equivalents of nitrate of soda and

sulphate of potash, or rather, nitrate of soda being eighty-five, and sulphate of potash eighty-seven, in a rough way equal quantities were taken, to save trouble with small weights. Thirty ounces of each were dissolved together and evaporated, until a slight pellicle appeared on the surface. After being allowed to crystallize for some days, there was found a quantity of saline matter, amongst which plenty of sulphate of potash could be seen, with a few small crystals on the top. One of these was picked out, and found to be sulphate of soda, instead of saltpetre; and, of course, if there was sulphate of soda, there must also be nitrate of potash. The mother liquor was evaporated, and a few small crystals of nitrate of potash formed on the top of more saline matter. In the memorandum of this trial the remark is made, "that the decomposition might be made perfect, but the conditions under which this may be effected are peculiar. It was tried to find out the peculiar condition, which was supposed to be some special density of the solution, or some stage to which the solution should be salted down, or some particular temperature at which the liquor should be kept until it crystallized; but I could not succeed in getting nitrate of potash or sulphate of soda again, or the slightest indication of a decomposition, although a good many trials were made. Many different degrees of density were tried, weak solutions set aside to evaporate spontaneously, the solution of one salt was gradually added to the other while being evaporated, and then the contrary; but I never could get as good an indication of decomposition as in the first little trial.

About two years afterwards, wishing to try to obtain some benefit from the 7 per cent. of sulphate of potash, I endeavoured to collect it by scraping it off the sides of the pan in which the saltpetre liquors were boiled down; but it turned out to be sulphate of soda, instead of sulphate of potash. This was somewhat tantalizing. How could sulphate of soda exist in presence of so much potash, after our former experience? But this fact revived the old notion; and I was again convinced that the decomposition could be effected if I could only discover the conditions, and that there must be something that induces the decomposition.

If we put some iron filings into water we have all the elements present from which we should get hydrogen, but we do not get it; when an acid is added the decomposition goes on at once—the acid induces the oxidation, freeing the hydrogen (as I do not suppose the acid attacks the pure metal first), and the decomposition we were aiming at seemed to require something like this inducing action. How would a larger quantity of the nitrate of soda, the very soluble salt, act? The

larger quantity might retain still more firmly the sulphate of potash in solution. Four ounces of nitrate of soda and two ounces of sulphate of potash were dissolved together and evaporated, until a quantity of saline matter was thrown down, which was allowed to settle for a few minutes, the clear liquor poured into a separate vessel, when nitrate of potash crystallized very readily; the boiled-down salt was then dissolved, and sulphate of soda was produced. It seemed now to require only an excess of the nitrate of soda to begin the decomposition, and the boiling-down process would complete it. Quantities of ten and twelve cwts. were now used, keeping the nitrate of soda in excess of the sulphate of potash by two cwts. After having about two tons of material in process, we could not get a decent crop of crude saltpetre: we got a mixture of sulphate of potash and sulphate of soda thrown down together by the boiling, and instead of getting crystals of saltpetre, we got a mixture of sulphates and nitrates of potash and soda. We had evidently not realized the true cause of the decomposition yet. I don't know what may now be considered the rationale of Le Blance's soda process, but the following is the diagram as it was understood thirty-seven years ago:—



The carbonic acid of the limestone is omitted for simplicity.

Theoretically, all above × this only are required for the decomposition of sulphate of soda; but when tried with these proportions the process was a failure; and it was only when another equivalent of lime was added that the process was successful. The idea that the same conditions were required in the case of the sulphate of potash presented itself—that instead of a slight excess of nitrate of soda, a double equivalent—two equivalents were absolutely necessary. The proportions of the materials in process were made up to two of nitrate of soda and one of sulphate of potash, and the decomposition was now found to be complete. After some trials, the following plan was adopted:—

Seven cwts. of sulphate of potash were dissolved in water in a pan capable of holding about 600 gallons, leaving room for the addition of

30 cwts. of nitrate of soda; a hot concentrated solution of sulphate of potash was prepared in another pan, and as the nitrate of soda solution evaporated, the solution of the sulphate of potash was added. The sulphate of potash liquor being weak—about 13° or 20° Twaddell—the process is somewhat tedious, but by-and-by sulphate of soda begins to form, which does not sink in the boiling liquor, but floats on the surface until a considerable quantity is produced. It is then fished out in the usual way. Care must be taken to arrange the quantities so as to have never less than two of nitrate of soda to one of sulphate of potash; practically, there was always a good deal more than two of the former. When a sufficient quantity of sulphate of soda was produced, by continuing the boiling for some time, at between 80° and 90° Twaddell, the whole is allowed to settle for two or three hours, and is then drawn over to crystallize the saltpetre; a good deal of sulphate of soda is left in the bottom of the pan, the fished out sulphate of soda is thrown back into the pan, dissolved in hot water, not boiled, and the whole turned into another vessel to crystallize the sulphate of soda.

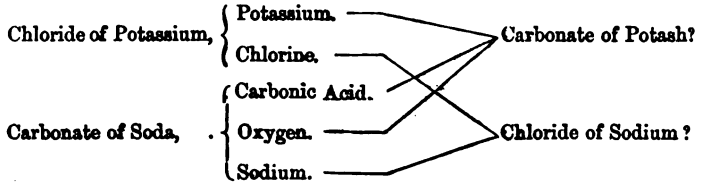
The sulphate of soda has quite a different appearance from common salt got in the same way: it is a peculiar, fine, white, soft powder, not the least granular or gritty, and dries very readily when taken out of the hot saltpetre liquor.

In the soda process there are so many combinations and changes in the manufacture, that there may be some difficulty in making out exactly the action which the extra equivalent of lime has; but in the saltpetre case it is very plain sailing, and I should like to get some explanation or reason for the fact that two equivalents effected the decomposition so readily, which it was quite impossible to do with the theoretical quantities. When indications of the decomposition were produced in the first trial, it was simply because, in taking equal quantities (in a rough way, to save trouble with small weights), there was over thirty grains more nitrate of soda in the solution than the equivalent corresponding to the sulphate of potash; and, in the trials afterwards, the salts were examined for their refraction, and the exact equivalent taken, and it was then found that the more correct we were with the quantities, the less chance was there of getting the result that was wanted.

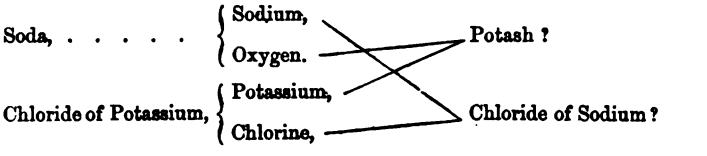
After this note had been written, in a conversation with Mr. Tatlock on the subject, he said he would like to see chloride of sodium tried instead of the second equivalent of nitrate of soda; that is, to dissolve and salt down one equivalent of nitrate of soda, one equivalent of sulphate of potash, and one equivalent of chloride of

sodium. I did not expect this would succeed, because I had some idea that the peculiar character of the sulphate of soda, as to its solubility, had something to do with the decomposition of the sulphate of potash—the highest point of concentration of sulphate of soda being at 90° Fahr., and as the heat is increased the anhydrous sulphate of soda is thrown down although little or no evaporation goes on, but on trying what Mr. Tatlock proposed I found it quite successful. I could not say that the decomposition was complete, but a good crop of sulphate of soda and nitrate of potash was got. Mr. Tatlock had noticed, when studying the Saltpetre process at Kames Gunpowder Mills, that although sulphate of potash was present in small quantity in the muriates used, that instead of sulphate appearing, it was always chloride of potassium, and that the sulphate of potash was decomposed by the common salt.

The decomposition of the sulphate induced the idea that the more valuable carbonate of potash might be made in the same way from muriate of potash thus:—



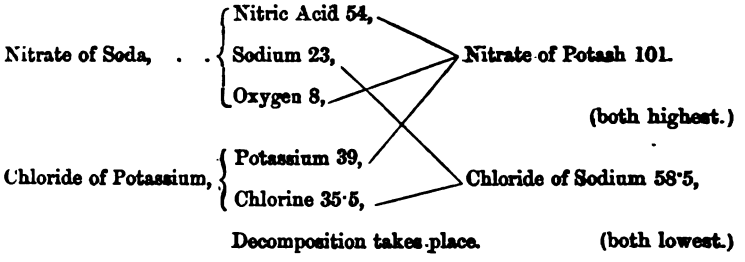
In this mixture the common salt, being the least soluble, we should expect it to be thrown down by boiling or salting—more especially as the great solubility of the carbonate of potash might induce the decomposition. A large excess of first one salt and then the other was tried, but I could obtain only the two salts as they were originally employed. The same thing was tried with caustic soda, which it would be difficult to throw down:—



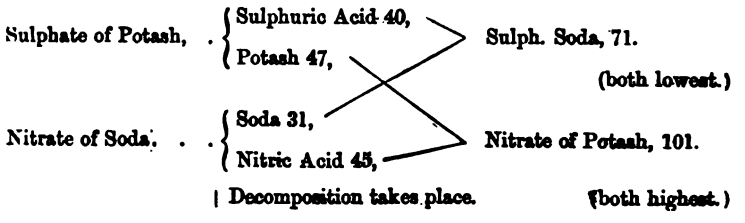
Chloride of sodium being much less soluble than chloride of potassium we might expect the common salt thrown down, but the first deposit was chloride of potassium. It may be worth while remarking that in the mixtures of these salts double decomposition only took place when

the base having the highest equivalent number was separated from the acid having the highest equivalent number:—

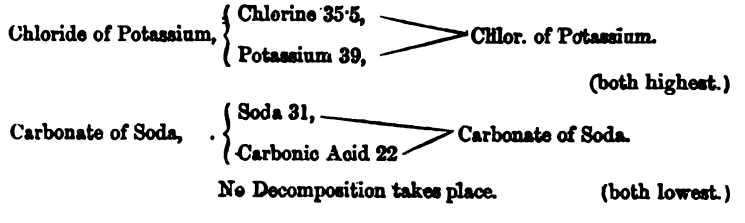
(1)



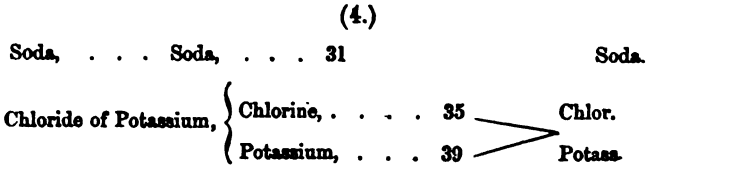
(2)



(3)

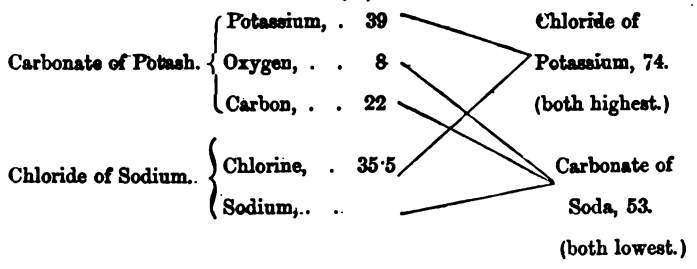


(4.)



No decomposition takes place. In these two cases the highest and lowest were already combined before mixing.

(5.)



The slight indication of a rule here might be examined further, but it may only appear in cases where the boiling down or salting process is used.

In making an analysis of commercial carbonate of potash, two chemists may arrive at exactly the same results, but the one may put a greater value upon the article than the other, simply by the different method of stating in making up the analysis. The one makes all potash available by putting to it all the carbonic acid, and puts to the soda the acids that would otherwise detract from the value of the potash. Now, it is evident from the examples (3) and (5), that all the soda must be reckoned as carbonate, and the sulphates and chlorides reckoned as sulphate of potash and chloride of potassium.

XXIII.—*On the Past and Present of Iron Smelting.*

By Mr. St. John V. Day.

Communicated to the Society, April 3, 1873.

Part I.

(a.) *Preliminary Remarks.*

As to the importance of the position which pig iron occupies in the list of our manufactures, it were idle to urge anything in explanation to a society located in Glasgow. When we consider that in 1871 no less than 16,859,063 tons of iron ores were smelted in Great Britain alone, from which was produced 6,627,179 tons of pig iron, representing a money value at the works of £16,667,947,* and which for the corresponding period we have just passed through, must by reason of an unprecedented demand for the material itself, and at unprecedented prices, be greatly increased; it will, I venture to hope, be readily admitted that our time may be profitably spent in considering the steps by which a manufacture, in former years carried on very much in the dark, has at length been reduced by the conjoint labour of many to *almost* a scientific exactitude. To say that iron smelting has yet been completely reduced to a science would be nothing other than pretence; nevertheless, that with a given furnace, ore, fuel, flux, and blast, we can estimate within tolerably narrow limits the quality and quantity of the product. Yet there are numerous points in the true understanding of what takes place in the blast furnace which are still enshrined in the region of uncertainty.

Within the last forty years, it may be said that iron smelting has been becoming by slow degrees to be scientifically understood, since Mushet and Clark in our own country, as well as several French and German physicists, have devoted their energies to the solution of various inquiries wherewith the subject is entangled; but since 1846, when the first furnace was built at the Walker Works, by

* Mineral Statistics, 1871.

Mr. I. Lowthian Bell, for smelting the Cleveland iron-stone,* and several more iron-making districts, with furnaces of colossal dimensions, have sprung up, the most important investigations, so far at least as our own country is concerned, have been carried out, the general results of which have led to improvements in practice, whereby the fuel required for smelting has been reduced by about 30 per cent.—this being directly due to operating with a larger bulk and higher column of materials at a time; utilizing the waste gases for heating the blast and generating steam for the blowing engines; and to a greatly elevated blast temperature.

No argument can be necessary to shew why it is important, in dealing with the subject of this investigation, to attack it at the very foundation; for that must be self-evident to any one whom it may concern to understand it, and as certain special reasons which, I trust, will clearly appear in the sequel, seem to render it desirable to consider briefly some information which comes to us from remote past ages, it may not, I hope, be considered tedious nor out of place if, at the commencement of this record, I dwell somewhat briefly on a few features in the history of the subject.

Any attempt at elucidating the course through which the modern gigantic operations of iron-smelting have been reached involves at once the history of the manufacture of cast iron—and it is not too much to say that recent investigations into that subject, if they prove anything at all, prove, amongst other things, that the true history of cast-iron still remains an unwritten chapter. However interesting, as well as useful it might prove, to probe the ultimate depths of that history, yet it is not proposed as a feature of this paper to attempt what must at present be so unfathomable a task.

Before entering into the deeper points to which the subject before us will probably be found to reach, I may remark that, whereas by some researches,† made a few years since, I was enabled to prove, from a variety of consentaneous evidence, that *malleable* iron was well known and used at least as far back as 4,000 years ago, and almost certainly much earlier still, I was thereby, and of necessity, led to doubt whether the usually accepted assertion as to cast iron having been invented within the last three or four hundred years only, rested on an entirely stable and reliable basis. The sequel will shew the results of the doubt so raised in my own mind.

* *Chemical Phenomena of Iron Smelting*, Preface.

† Vide *Proc. Phil. Soc.*, Glasgow, Vol. vii., p. 476.

(*b.*) *The Origin of the Blast Furnace.*

Not unlike many other discoveries made at periods remote from the present age, and which have had in varied degrees incalculable influence upon the condition and destinies of mankind, does it at a first view appear out of keeping with an almost constant order, that the place and date, no less than the names, of the first makers of cast iron are not absolutely known.

When, however, we reflect upon that which we really do know, as being reliably ascertained concerning early methods of making iron and steel, weigh carefully the precise nature of the conditions involved under those methods, and seek out the results inevitably accruing through them, as explained by the guiding light of modern chemistry, it would appear that the blast furnace as a distinct apparatus could scarcely at any time have consisted in a definite or sudden departure from an existing order of things; by saying which, I mean to explain, that in all probability, there never was in the development of iron smelting an *immediate complete change* made from the method of reducing ore at once to malleable metal (the direct method) to that of first making pig or sow metal (or the indirect method of the blast furnace as we practise it to-day); rather, on the contrary, the evidence which has been collected goes to shew that the blast furnace was ultimately reached as a definite and distinct apparatus for reducing iron ore quickly, and producing an easily fusible compound of iron, partly by its accidental production occasionally when reducing easily fusible ores in the air or blast bloomeries, or other formerly used types of *low* furnaces, in which the product sought to be obtained was malleable iron or steel. This probability, indeed, appears to rest on conclusive grounds; and the tendency of the evidence is further to shew that the blast furnace, as an apparatus having as a distinct object the production of cast iron, was at last arrived at through very gradual accessions to the height of the ancient types of low furnaces.

Where we are to look for the earliest traces of the practice of reducing iron to the form of a carburet or as cast iron, I cannot suppose that at the present time any one would venture to assert; but as the employment of steel in fashioning the stones used in the monuments of Proto-Egypt, India, Greece, and elsewhere, has been shewn, that almost seems to imply the acquaintance of those ancient nations with the *fusion* of iron, and leads us to expect that to the East and not the West must we look for the beginnings of the art.

In so far as our own country has yet given testimony, the oldest

blast furnaces yet recorded are those of which the ruins formerly existed, and may, for aught I know, still exist, in the Forest of Dean, and the age of which Mr. Mushet has computed as belonging to the commencement of the seventeenth century.*

* In his "Papers on Iron and Steel," Mr. Mushet supplies us with the following instructive remarks:—"I have examined the sites of many old charcoal blast furnaces, with a view of determining their age, by the quantity of slags with which they were surrounded. Here, however, another difficulty has been, in every case but one, interposed. The manufacture of black bottles has, I think, been traced as far back as the fifteenth century. At what time the manufacture was introduced into this country, I am uncertain; but it is not improbable that in early times, as in the last century, the slags or cinders of the charcoal blast furnace have entered into the composition of black bottles, and created a consumption of that sort of waste which otherwise would have remained in the vicinity of the furnaces. The superior quality of the Bristol black bottles has been attributed to the immemorial use of a portion of the slags of the charcoal furnaces from the neighbourhood of Dean Forest. The consequence of this long-standing practice has been to carry from the furnaces not only the old slags, but those currently made. In one instance only have I found from this source data for calculation. Before the civil commotions of the seventeenth century, the Kings of England were possessed of two blast furnaces in the Forest of Dean, when the cord-wood of the Forest and the king's share of the mines were used for the purpose of iron-making. Soon after the commencement of the struggle between Charles the First and his Parliament, these furnaces ceased working, and at no period since have they been in blast. About fourteen years ago, I first saw the ruins of one of these furnaces situated below York Lodge, and surrounded by a large heap of the slag or scoria that is produced in making pig iron. As the situation of this furnace was remote from roads, and must at one time have been deemed nearly inaccessible, it had all the appearance, at the time of my survey, of having remained in the same state for nearly two centuries. There existed no trace of any sort of machinery, which rendered it highly probable that no part of the slags had been ground (the usual practice) and carried off, but that the entire produce of the furnace in slags remained undisturbed.

"The quantity I computed at from 8,000 to 10,000 tons; a quantity which, however great it may appear for the minor operations of an early period, would yet in our times be produced from a coke furnace in less than two years. If it is assumed that the furnace made annually 200 tons of pig iron; and further, assuming the result which has been obtained with ores richer than the Roman cinders, and ores used at that time in Dean Forest, *that the quantity of slag run from the furnace was equal to one-half of the quantity of iron made* (in modern times the quantity of cinder from the coke furnace is double the weight of the iron), we shall have 100 tons annually for a period of from 80 to 100 years. If the abandonment of this furnace took place about the year 1640, the commencement of its smeltings must be assigned to a period between the years 1540 and 1560." Mushet, from this computation, assigns the mean period or 1550 as the most probable period for the commencement of smelting operations with this furnace. In a note, however, at the end of the paper from which the previous

It is desirable, ere proceeding too far in the paths of research which for the present occupy our attention, in order to avoid any

extract is taken, he says, "the calculation of age, which proceeded on the assumption of a certain weight of cinder being obtained in the production of a given weight of iron, and which with so rich ore may be correct; yet, on further consideration of the subject, and taking into account the calcareous nature of the iron ores of Dean Forest requiring a considerable portion of argillaceous schist to neutralise the lime, it is more than probable that the furnace would necessarily, from this circumstance, and from the inferior produce of the ores, produce fully as much cinder as pig iron, and that in place of only being one-half the weight, it would probably be of equal weight with the iron. Taking the calculation in this way, we should not reach an older period than the commencement of the 17th century for the introduction of the blast furnace into Dean Forest.' . . . The local history of Tintern Abbey assigns a later period (the early years of James the First) for the erection of that furnace. The opportunity afforded of examining both the slags and the iron produced in that early period abundantly proves that the furnace in Dean Forest above mentioned was one of the earliest efforts in the art of making pig iron. Small masses or shots of iron are found enveloped in the slags, specimens of iron in a malleable state, though rarely, more frequently rough nodules of large grained steel, resembling blistered steel, and others of a more dense fracture, but of a similar quality. The more fusible reguli of white, mottled and grey iron are found in great abundance, all of them possessing forms and appearances of fusion more or less perfect, according to the quantities of carbon with which they are united; and it is but justice to the memory of the fathers of this art to add, that the specimens of grey cast iron are more abundant than those of the other sorts.

"This furnace seems to have been erected upon the spoils of former ages of iron-making; and probably the situation was in the first instance determined by the numerous bloomeries that existed in the neighbourhood—the scoria of which has in after ages been worked to so much advantage in the blast furnace; and though, as a blast furnace, possessed of no great antiquity, yet, as the site of the ancient bloomery, entitled to be considered as the remains of an extensive manufactory of iron in ages more remote.

"Upon the whole, several circumstances incline me to the opinion, that the blast furnace must have been known in some of the then iron-making districts of England before it was introduced into Dean Forest. The eldest casting I have met with in Dean Forest is dated 1620.

"The great infusibility and difficulty attending the management of calcareous ores, such as those belonging to Dean Forest, is another circumstance that inclines me to think that the art of making pig iron did not originate in that quarter, and probably did not succeed entirely till the practice of increasing their fusibility by the addition of the bloomery cinder became known and established. These conjectures are confirmed by reference to a paper in my possession, professing to be an account of all the blast furnaces in England previous to the manufacture of pig iron from pit-coal—probably about the year 1720 or 1730; in which, however, the blast furnace of Tintern Abbey is omitted, and possibly others. At that period there were in all England 59 furnaces,

necessity for raising the question hereafter, once and for all to point out, that, it is not a consequence, because we are unable to assign an earlier positive date for the blast furnace than that above given, that cast iron was unknown before that period; indeed, from what we do glean from the historical records, they assure us that it was in considerable use at a much more remote age. And whereas this knowledge might lead some persons to conclude that as the blast furnace constitutes the first step taken in the manufacture of cast iron to-day, it was necessarily the first step taken in ages long past; still, a candid consideration of certain features of history, coupled with a consideration of what chemistry now teaches, are more than sufficient to convince us that the high or blast furnace is not indispensable to the production of that carburet, however much it is essential, under our current knowledge at the present period, in order to comply with modern demands for the metal at paying prices.

To but briefly, indeed, indicate how much more ancient cast iron may really be than, so far as I have ascertained, has been noticed during the last quarter of a century,—a period unprecedented in the issue from the press of a metallurgical literature of extreme value,—I may mention a process of making steel used by the

making annually 17,350 tons, or little more than 5 tons of pig iron a week for each furnace.

"Should it appear that there have been since the invention of blast furnaces iron-making districts in England in which a greater number of furnaces have been established than in Dean Forest, then to that quarter I should be inclined to look for information on the history, rise, and progress of the blast furnace.

Brecon, . .	2	Gloucester, .	6	Salop, . . .	6
Glamorgan, . .	2	Hereford, . .	3	Stafford, . .	2
Carmarthen, .	1	Hampshire, .	1	Worcester, . .	2
Cheshire, . .	3	Kent, . . .	4	Sussex, . .	10
Denbigh, . .	2	Monmouth, .	2	Warwick, . .	2
Derby, . .	4	Nottingham, .	1	York, . . .	6

"It would appear from this account, that the counties of Sussex and Kent alone contained, in the early part of the eighteenth century, 14 blast furnaces; and as it is probable that the woodlands in the vicinity of the metropolis would sooner disappear than in the more distant counties, it is equally probable that a century before the number of blast furnaces might have been considerably greater in that district. The only other iron-making district that will at the time now spoken of bear a comparison with Sussex and Kent, is that of Dean Forest, in which I include the Furnace of Tintern Abbey, in Monmouthshire, not included in the list; Gloucestershire 6, and Herefordshire 3,—making in all 10 blast furnaces."

Greeks, and recorded in the writings of no less an authority than Aristotle, and to which I have, on a previous occasion, directed attention.* Where it is stated:—

"Wrought iron itself may be *cast* so as to be *made liquid, and to harden again.*"

Somewhat obscure as the Aristotleian account of Greek steel manufacture unquestionably is, nevertheless, when the terms of the fragment are analyzed, and it is placed in juxta-position with other accounts of steel-making appertaining to times long subsequent, it is even sufficient to assure us that such iron, although it may not have been specially employed in the art of making castings, but produced for the purpose of converting bars of wrought iron into steel, by a process of cementation in a bath of metal surcharged with carbon, was known to and practised by the Greeks at least as early as 400 years before our era.

Indeed, we may venture further still—for recent discoveries in India, and the impossibility of explaining Egyptian sculpture in granite, porphyry, diorite, &c., without the use of steel tools, hold out much to hope for towards the increasing of our acquaintance with the metallurgy of the ancient eastern world, by further special researches into the storehouses of information yet waiting there to be opened up. For, after the discovery of the Kutub Minar Lâht,† near Delhi, as well as the huge iron beams in the Temple of Kanaruc,‡ and the coming to light of numerous other testimonies, proving beyond doubt the extremely high acquaintance with manufacturing art, which some persons at least possessed in the East in ages long past, the cautious observer is compelled to pause ere risking to pronounce, whether, as it even yet is generally asserted, Western civilization has in all respects exceeded all previous civilization, or questioning, whether we have attained in some respects the position in certain of the manufactures most important to man at one time reached in the old world; for, whilst the rate of production has increased as a necessary sequence of the growth of population, and novel as well as wider fields of application, yet it is notorious that in many instances high quality is not maintained. There is much to be met with in the remains of the Proto-Egyptian, Assyrian, Greek, and Chinese nations to assure us that we have not—while to Central Asia, Asia Minor, and Persia we must look hopefully for further light in this respect.

* Vide *Proc. Phil. Soc., Glasgow*, vol. viii., p. 244.

† *Trans. Asiatic Soc.*, Bengal, 1864.

‡ *Illust. Ancient Architecture of Hindustan*, p. 28, Pl. iii., 1848.

With this much of digression from the immediate subject in hand, purposely introduced too as a forewarning signal to us that at this time we have no sufficient facts to warrant us in assigning any approximate period even for the origin of the indirect method of reducing iron ores (the prevalent system of this age), we may with advantage return to the question of producing cast iron *without the* blast furnace; in order to satisfy ourselves that, whilst all the very old examples of iron which we do find are malleable, and appear from more than one point of view to have been produced from ores reduced *without fusion;* and when inquiring still further into the most ancient practice of reduction, no country so far affords conclusive evidence of cast iron having been an established manufactured product—in the sense we find malleable iron to have been therein—yet the collateral evidence as to an extremely early method of making steel, in the production of which cast iron was a *sine qua non*, convinces us of the necessity for exercising extreme caution ere drawing a conclusion.

The next early intelligible account that we have of steel-making throws equal light over cast iron making, and this is to be found in a work entitled "*De la Pirotechnia*," published at Venice in 1540, by Vanoccio Biringuccio; and in the somewhat later, but better known writings of Agricola—"*De re Metallica*"—published about 1561. Both these authors describe a process of converting bars of malleable iron into steel by keeping the bars immersed for a considerable time *in molten cast iron.*

The process as described by the earlier author has been translated by Mr. Panizzi,* of the British Museum; and I here quote an extract from that translation, shewing how the cast iron was produced.

"Steel is nothing but iron well purified by means of art, and through *much liquefaction* by fire brought to a more perfect admixture and quality than it had before. By the attraction of some suitable substances in the things which are added to it, its natural aridity is mollified by somewhat of moisture, and it is made whiter and denser, so that it seems to be almost removed from its original nature; and at last, when its pores are well dilated and mollified with much fire, and when the heat is driven out of them by the extreme coldness of the water, they contract, and so the iron is converted into a hard substance, which from its hardness becomes brittle. This may be done with every kind of iron, and so steel

* *Metallurgy, Iron and Steel.* By John Percy, M.D., F.R.S., London, 1864, Murray, p. 807, *et seq.*

may be made of all kinds of iron. It is true, indeed, that it is made better from one kind than from another, and with one sort of charcoal than another, and it is also made better according to the skill of the masters. The best iron to make it good is, however, that which, being by its nature free from the corruption of any other metal, is more easy to melt, and which is to a certain extent harder than other kinds. With this iron is put some pounded marble or other fusible stones, in order to melt them together. By these it is purged, and they have, as it were, the power of taking away its ferruginosity, of constricting its porosity, and of making it dense and free from cleavage. Now, to conclude, when the masters wish to do this work, they take of that iron passed through the furnace or otherwise as much as they wish to convert into steel, and they break it into little bits; then they prepare before the aperture of the forge a circular receptacle, about a foot or more in diameter, made of one-third clay and two-thirds small coal (carbonigia), well beaten together with a hammer, well mixed, and moistened with so much water as will make them keep together when squeezed in the hand; and when this receptacle is thus made, in the same way as they make a hearth (ceneraccio), but deeper, the aperture is prepared in the midst, which should have a little of the nose turned down, so that the wind may strike in the midst of the receptacle. Then, when all the space is filled with charcoal, they, moreover, make round about it a circle of stones or soft rock to keep in the broken iron and the additional charcoal which they put upon it, and so they fill it up and make a heap of charcoal over it. Then, when they see that the whole is on fire, and well kindled, especially the receptacle, the masters begin to set the bellows to work, and to put on some of that crushed iron mingled with saline marble and with pounded slag, or with other fusible and not earthy stones; and so *melting* this composition by little and little, they fill up the receptacle so far as they think fit; and having first formed with the hammer three or four lumps of the same iron, each weighing 30 or 40 lbs., they put them hot into that *bath of melted iron*, which bath is called by the masters of this art the art of iron; and they keep them thus in the midst of this melted matter with a great fire about four or six hours, often turning them about with a rod as cooks do victuals, and so they keep them there, turning them again and again, in order that all that solid iron may receive through its porosity those subtle substances which are found to be within that *melted* iron, by virtue of which the gross substances which are in the lumps are consumed and dilated, and the lumps

"become softened, and like a paste. When they are seen thus by the experienced masters, they judge that that subtle virtue of which we have spoken has thoroughly penetrated; and taking out one of the lumps which appears best from their experience in testing, and bringing it under the hammer, they beat it out, and then throwing it suddenly as hot as they can into the water, they temper it, and being tempered, they break it, and look to see if the whole of it has in every particle so changed its nature as to have no small layer of iron within it; and finding that it has arrived at that point of perfection which they desire, they take out the lumps with a large pair of pincers, or by the ends left on them, and cut them into small pieces of seven or eight each, and they return them to the same bath to get hot again, adding to it some pounded marble and *iron for melting* to refresh the bath and increase it, and also to restore to it what the fire may have consumed, and also that that which is to become steel may, by being immersed in that bath, be the better refined; and so at last, when these are well heated, they go and take them out piece by piece with a pair of pincers, and they carry them to the hammer to be beaten out, and they make rods of them as you see. And when this is done, being very hot, and almost of a white colour from the heat, they cast them all at once into a stream of water as cold as possibly can be had, of which a reservoir has been made, in order that the rods may be suddenly cooled, and by this means get the hardness which the common people call temper, and thus it is changed into a material which hardly resembles that which it was before it was tempered. For then it was only like a lump of lead or wax, and by tempering it, it is made so very hard as almost to surpass all other hard things; and it is also made very white, much more so than is the nature of its iron, even almost like silver, and that which has its grain white, and most minute and fixed, is of the best sort. Among those kinds which I know of, that of Flanders, and in Italy that of Valcamonica, in the territory of Brescia, are very much praised; and out of Christendom, that of Damascus, that of Caramenia and Lazzimino (?), as well as that of the Agiambi (?)."

The same process is described by Agricola; but it is worthy of remark, as stated on the authority of the elder Mushet, that "nowhere does he describe a process by which cast iron was obtained and applied to *foundry* purposes."*

In India, near Trincomalee, steel (wootz) is still made in the same manner, its manufacture being confined to a few families in that

* *Papers on Iron and Steel*, London, 1840, p. 380.

neighbourhood, and altogether unknown to the common steelmakers of Salem, a distance of only 70 miles. The cast iron used in this case is obtained from "a small blast furnace, about 8 feet high, and tapering from 18 inches diameter at the bottom to 9 inches at the top. The iron flows out of a grey quality, but without perfect separation, as the cinders produced contain a good deal of iron.

With regard, then, to the production of cast iron in the most ancient *low* furnaces, that was practicable with ores not difficult to fuse when in presence of large quantities of flux and a great excess of charcoal—the former of which would preserve the metal from oxidation, whilst it was allowed to remain a sufficient time in contact, to take up a maximum quantity of carbon from the latter; but as the temperature in such furnaces was low, the slag of necessity contained a large proportion of the iron, and, except with the most easily fusible ores, the process was very slow; indeed, with the more difficult fusible ores, almost impossible. With this certainty before us, however, of the possibility of producing cast iron even in the oldest known types of furnaces, coupled also with the well-ascertained fact of the use of iron and steel by Greeks, Indians, ancient Egyptians, and Assyrians,† it is impossible to say how far back we may carry the date of the discovery of cast iron. But it is not, as I have already pointed out, to be inferred that the blast furnace has any claim at all to antiquity; on the contrary, I have collected together the foregoing evidence with the one object, amongst others, of avoiding any misapprehension on that point.

Percy,‡ remarking on a quotation from Lower's *Contributions to Literature, &c.*, says,—

"The date of the discovery of cast iron has not, so far as I am aware, been precisely ascertained, though it is a point of great archæological interest. Lower has published the following remarkable statement, which would lead to the conclusion that cast iron was made and applied in England 500 years ago. A curious specimen of the iron manufacture of the fourteenth century, and, as far as my own observation extends, the oldest existing article produced by our foundries, occurs in Burwash church (Sussex). It is a cast iron slab, with an ornamental cross, and an inscription in relief. In the opinion of several eminent antiquaries, it may be

* *Papers on Iron and Steel*, London, 1840, p. 673.

† *Proceedings Phil. Soc.*, Glasgow, vol. vi., 1871; also *Trans. Devon. Assocn.*, 1868.

‡ Percy's *Metal; Iron and Steel*, p. 878.

regarded as unique for the style and period. The inscription is much injured by long exposure to the attrition of human feet. The letters are Longobardic, and the legend appears, on a careful examination, to be,—

'ORATE P. ANNEMA JHONE COLINE, (or Colins).

'Pray for the soul of Joan Collins.'

Of the identity of the individual thus commemorated I have been unable to glean any particulars. In all probability she was a member of the ancient Sussex family of Collins, subsequently seated at Locknersh, in the adjacent parish of Brightling, where, in company with many of the neighbouring gentry, they carried on the manufacture of iron at a place still known as Locknersh Furnace."

M. Verlit says that cast iron was known in Holland in the thirteenth century, and that stoves were made from it at Elass, in 1400, A.D.;* and, according to Lower, the first cannon of cast iron were manufactured at Buxteed, in Sussex, by Ralphe Hogge, in 1543. It is recorded, however, by others that the first iron guns cast in England were made in London, in 1547, by Owen; and in 1595 the art of iron casting was so well understood that John Johnson and his son Thomas had by that time "made forty-two cast pieces of great ordnance of iron for the Earl of Cumberland, weighing 6,000 pounds, or three tons a-piece." Agricola, too, who died in 1494 A.D., seems to have been acquainted with cast iron; for he writes,—"Iron melted from ironstone is easily fusible, and can be *tapped off;*" so that although he does not appear to say anything as to the method by which such cast iron was produced, it nevertheless is evident, when we consider the large extent to which cast iron was probably then employed for guns, and doubtless other purposes, that the blast furnace was at that time in existence, though on a very small scale, grown out of the Catalan, and through the Blaseofen, or Osmund,† to the German Stückofen, in which cast or malleable iron

* Mushet's *Papers on Iron and Steel*, p. 391.

† Percy says (*Iron and Steel*, p. 320),—"Between the Luppenfeuer, or Catalan furnace, and the Stückofen, German metallurgists place a furnace of intermediate height, which they designate Blaseofen and Bauernofen. This furnace was formerly employed in Norway, Sweden, and other parts of Europe; and although a century may have elapsed since it became extinct in the first two countries mentioned, yet to this day it continues in operation in Finland." "Osmund" is the Swedish word for the bloom produced in this particular kind of furnace, of which the annexed woodcuts (Figs. 1 and 2) are a plan and vertical section, respectively, shewing the outside as consisting of a timber casing,

was produced as required, by varying the proportions of the materials constituting the charge.

"Osmund" Furnace.

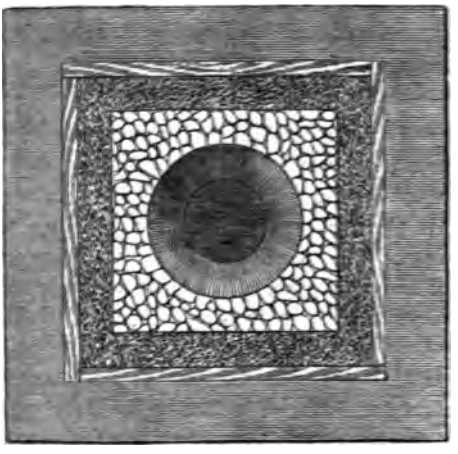


Fig. 1.—Plan.

As the Stückofen would appear to be the last stage of transition from the *low* to the *high* furnace, into which it ultimately became

"Osmund" Furnace.

Fig. 2.—Section.

merged altogether, when the discovery was made that the ore was more completely reduced, and the variety of purposes to which

and the inner part a lining of refractory stone, the space between them being filled with earth.

The Osmund furnace is used for reducing the hydrated sesquinoxide ores (lake or bog iron ores) found in the lakes and rivers of some parts of Northern Europe, and in Finland is stated at the present day to be working side by side with the modern blast furnace.

the pig or sow metal could be applied increased the demand for cast iron to such an extent as to induce the indirect method of reduction to be carried out on a large scale, it will be unnecessary in this paper, which deals with cast iron and the blast furnace as its principal subjects, to refer further to the pre-existing *low* furnaces.

Regarding the Stückofen, then, or high bloomery furnace, it has been correctly described by writers on metallurgy as a Catalan or low furnace, extended upwards in the form of either a circular or quadrangular shaft. In Germany this furnace is also known as Wulfsofen, the reduced metallic mass resulting from the operations being designated "Stück" or "Wulf:" hence the Stück or Wulf oven—Salamander* furnace—for the following particulars of which I am indebted to Professor Osborne's treatise,† and who, in a paragraph preceding the extract, significantly terms this the "transition furnace," which might be used for the production of cast iron or malleable iron at will, by varying the constituents of the charge and the intensity of the blast.

Osborne says,—

"This kind of furnace is at present very little in use. A few are still in operation in Hungary and Spain. At one time they were very common in Europe. The iron produced in the Stück oven has always been of a superior kind favourable for the manufacture of steel; but the manipulation which this oven requires is so expensive that it has been superseded. Fig. 3 shews a cross section of a Stück oven; its inside has the form of a double crucible. This furnace is generally from 10 to 16 feet high, 24 inches wide at bottom and top, and measures at its widest part about 5 feet.

The "Stückofen."

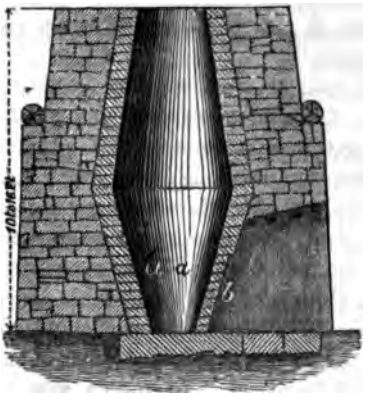


Fig. 3.—Section.

* "Salamander is the term now given to the mass of half-pure iron, which results when the molten mass of a furnace chills before it can be regularly tapped off into pigs. It is difficult to melt, and is sometimes largely malleable iron. The present may have originated from the earlier use of the word as applied to this furnace.

† *The Metallurgy of Iron and Steel*, Theoretical and Practical, in all its branches,

There are generally two tuyeres* [twè-er, allied to tuyaw, a pipe], *a a*, and at least two bellows and nozzles, both on the same side. The breast, *b*, is open, but during the smelting operation it is shut by bricks; this opening is generally 2 feet square. The furnace must be heated before the breast is closed; after which charcoal and ore are thrown in. The blast is then turned into the furnace. As soon as the ore passes the tuyere, iron is deposited at the bottom of the hearth; when the cinder rises to the tuyere, a portion is suffered to escape through a hole in the dam, *b*. The tuyeres are generally kept low upon the surface of the melted iron, which thus becomes whitened. As the iron rises the tuyeres are raised. In about 24 hours one ton of iron is deposited at the bottom of the furnace. This may be ascertained by the ore put in the furnace. If a quantity of ore is charged sufficient to make the necessary amount of iron for one cast, a few dead or coal charges may then be thrown in. The blast is then stopped, the breast wall removed, and the iron, which is in a solid mass, in the form of a salamander or "*stück-wulf*," as the Germans call it, is lifted loose from the bottom by crowbars, taken by a pair of strong tongs, which are fastened on chains suspended on a swing-crane, and then removed to an anvil, where it is flattened by a tilt hammer into 4-inch thick slabs, cut into blooms, and finally stretched into bar iron by small hammers. Meanwhile the furnace is charged anew with ore and coal, and the same process is renewed.

"By this method good iron as well as steel may be furnished. In fact, the salamander consists of a mixture of iron and steel—of the latter, skilful workmen may save a considerable amount. The blooms are a mixture of fibrous iron, steel, and cast iron. The latter flows into the bottom of the forge fire, in which the blooms are re-heated, and is then converted into bar iron by the same method adopted to convert common pig iron. If the steel is not sufficiently separated, it is worked along with the iron. This would be a very desirable process, on account of the good quality of iron which it furnishes, if the loss of ore and waste of fuel it occasions were compensated by the price of bar iron. Poor ores, coke, or anthracite coal, cannot be employed in this process. Charcoal made from hardwood, and the rich magnetic, specular, and sparry ores are almost exclusively used."

It is obvious that the conditions necessary to the production of

edited by H. S. Osborne, LL.D., Professor of Mining and Metallurgy in Lafayette College, Easton, Pennsylvania. Trübner & Co., London, 1869.

* One tuyere, however, is frequently used.—S.J.V.D.

cast iron—viz., a column of materials which gradually become increased in temperature during their descent, exposed to reducing gases, and latterly, prolonged contact in the reduced state to carbonizing matter, obtained in this furnace; and the result frequently was that, when intending to produce malleable iron at once, the lump was so much carbonized, owing to excess of carbonizing materials, that it had to be submitted to a decarbonizing process before it could be hammered. Experience in working the Stückofen proved it to be extremely wasteful of fuel; and about 1840 it was to a great extent abandoned in Carniola, Carnithia, and Styria, although still worked in Germany and Hungary to a limited extent (KARSTEN). In some cases a throat was added to the furnace, of a gradually widening form: this gave facility in charging. The tuyere was placed about a foot above the hearth bottom; but as the furnace continued in operation this distance became increased, by reason of the disintegration or wear of the hearth (silicious conglomerate), which we learn influenced the yield and quality of the iron as well as the quantity of charcoal consumed. Besides being made of the form shewn at fig. 3, the Stückofen sometimes increased with a regular taper throughout the entire height of the shaft, being broadest at the bottom, and both rectangular as well as circular in horizontal section. The tuyeres were sometimes made of clay, at others of copper, situated at different parts of the furnace; and when in the breast, the bellows had to be removed before the lump of reduced iron could be withdrawn. As the demand for cast iron increased, the Stückofen was gradually replaced by the Blauofen,* in which cast iron was produced alone; but it still retained its place for the direct production of malleable iron—and indeed malleable iron was also produced in the Blauofen, which at first, it would appear, was simply a tall Stückofen, eventually becoming increased in height to from 20 to 25 feet, in which case it was capable of producing cast iron only. In working these furnaces for the production of malleable iron, the slag was allowed a constant escape, so that the lump of metal in the hearth might be exposed to the action of the blast, which prevented it from becoming carbonized to excess; at other times the slag was allowed to accumulate, thus protecting the metal from the decarbonizing action of the blast, after it had become carbonized in passing through the lower part of the furnace, and therefore producing

* By some authors termed "blue furnace." Fr. "Fourneau bleu," "blue oven."

carbonized or cast iron. The Blauofen, as in common use on the continent, is represented in vertical section at fig. 4, wherein *a* is the breast, *b* the tuyere. This furnace may be kept in blast for three to six months, or even longer, when the hearth widens and interferes with successful operations. In working with this furnace, the practice is to heat it by a fire, after which the breast previously open is closed; it is then filled to the top with coal and iron ore, which are renewed as the charge sinks. The tuyeres are about 14 inches above the hearth, which slopes towards the breast. This furnace requires rich ores and a plentiful supply of charcoal, and produces good pig iron, as well as a metal specially suitable for steel, sometimes called "steel metal,"* and said to be that from which German steel (shear steel) is made. The management of the Blauofen is simple—generally and where sparry carbonates are plentiful—and the furnace is cheaply constructed.

The "Blauofen."

Fig. 4.—Section.

From the preceding remarks we have become familiar with the earliest known form of the blast furnace, which originating in the Stückofen, or high bloomery, of some 95 cubic feet capacity, passed into the Blauofen of some 500 to 600 cubic feet; and without following its progressive development minutely through the furnaces in the Hartz, Silesia, Prussia, Sweden, Great Britain, and America—all of which has been already done, and so excellently in the *Treatises* of Percy, Osborne, and others—we may at once come down to our own age, and now find furnaces in the Cleveland district of the enormous capacity of 20,000 to 30,000 cubic feet, or about 280 times that of an early Blauofen.

* Osborne's *Metallurgy*, p. 294.

MINUTES.

November 6, 1872.

THE Session of the Philosophical Society of Glasgow was opened this evening, Professor Grant, LL.D., the President, in the Chair.

Mr. James M'Ilwraith, Mr. James M'Gregor, Mr. John Donald, and Mr. Robert Downie, Junior, were elected members of the Society.

Mr. George Watson and Mr. James Reid were appointed Auditors of the Treasurer's Accounts.

The President delivered an Opening Address, for which, on the motion of Mr. Wünsch, he received the thanks of the Society.

Mr. James Thomson suggested that measures should be adopted this Session by the Society, along with other Scientific Societies in the city, to invite the British Association to hold an early meeting in Glasgow.

Dr. Fergus remarked that such a movement on the part of the Society would no doubt meet with the cordial co-operation of the public authorities.

Mr. James R. Napier expressed a desire that the attention of the Council should be directed to the Exhibition Fund in possession of the Municipal Corporation, with a view to the Society's obtaining it for the purpose of providing adequate accommodation.

November 20, 1872.

The Philosophical Society of Glasgow met this evening for the Seventy-first Annual Election of Office-bearers, the President in the Chair.

The following were elected members of the Society, viz:—

Mr. William Notman Peden; Mr. E. J. Scott; Mr. Robert M. Buchanan; Mr. Robert Grieve; and Mr. Benjamin Taylor.

The Annual Report by the Council on the State of the Society was read by the Secretary, as follows, and was approved of.

Report of the Council on the State of the Society.

1. *State of the Membership.*—At the commencement of the Session of 1871-72 the number of members on the roll of the Society was 510, and fifty were admitted in the course of the Session, making in all 560. From this number there fall to be deducted—three for arrears of subscription, eleven resigned, seven left the city, and six dead, in all twenty-seven—which number reduces the members on the roll at the opening of this Session to 533. Of this number only three are in arrears of subscription for one year. The remark in last year's Report holds equally true of the present state of the membership, that the number on the roll is the largest, and the arrears of payment, so far as is known, the least, in any year of the Society's existence.

2. *The Proceedings.*—The printed *Proceedings* of the Society in the Session 1871–72 occupied 234 pages, forming the first part of the eighth volume.

The opening address by Dr. Bryce, the retiring President, embraced, along with notices of deceased members, a review of the recent progress of science. The Inaugural Address of Professor Grant, LL.D., F.R.S., the President elect, was on "The Astronomy of the Nineteenth Century." The third contribution to the *Proceedings* was made by the Section of Sanitary and Social Economy in the form of a comparison of the Vital Statistics of the different districts of Glasgow, for the years 1869-70, by Mr. Alexander Scott. At the first meeting in January, Dr. Robert Bell read a paper on "Biogenesis, as bearing upon Decomposition, Putrefaction, and Decay." This paper was printed in a slightly abridged form in the *Proceedings*. By an arrangement with the Section of Sanitary and Social Economy, the Society had the benefit of a paper by Mr. John Phillips, C.E., London, "On the Drainage and Sewage of Towns," read in February, and printed at large. The other printed papers were the following:—"On Pressure Logs for Measuring the Speed of Ships," by Mr. James R. Napier, F.R.S.; read in March; in the same month Dr. Thorpe described the Results of the Recent Solar Eclipse. The Chemical Section, as usual, furnished a series of important contributions, viz.:—"On the Analysis of Chrome Ore," by Dr. John Clark; "On some Experiments with Fehling's Copper Solution," by Mr. T. L. Patterson; "On a Double Sulphide of Gold and Silver," by Mr. M. M. Pattison Muir; "On the Action of Dilute Saline Solutions upon Lead," also by Mr. Muir; "On the Jargons and Zircons

of Ceylon," by Mr. M. H. Cochrane; "On the Separation of Phosphoric Acid, Ferric Oxide, Alumina, Lime, and Magnesia," by Mr. Thomas Robertson Ogilvie; "On the part which Ferric and Aluminic Oxides play in the Manufacture of Superphosphate, and on the Comparative Value of Mineral Phosphates," by Mr. T. L. Patterson.

The following papers brought before the Society are not included in the printed *Proceedings*:—"Notes on the Tailless Trout of Islay," by Mr. James Thomson; "Notices of recent Geographical Discovery," by Dr. Bryce; "The Projectile Theory, as connecting Astronomy with Geology and Geography," by Mr. Charles Randolph; the President, "On the Indications we at present possess respecting the Extent which the Meteoric System occupies in the Physical Universe;" Dr. A. K. Irving, "On a New Form of Singing Flame, and its Application to a new Miner's Safety Lamp, to Audible Signals, and for Indications of Danger;" Mr. Edward, C. C. Stanford, "On a New Monochromatic Flame, and a Chemical Imitation of Lightning."

The *Proceedings* contain reports of the business transacted throughout the Session by the Chemical Section, the Section of Sanitary and Social Economy, and the Architectural Section.

At the Society's meeting on 24th January, the sum of £20 was voted to the Livingstone Search Fund.

The Council have again to acknowledge the valuable assistance they have received from their Finance Committee, in the administration of the Society's funds,—which Committee has met at stated times for the purpose of examining and passing accounts, and regulating the outlay of money.

The Abstract of Treasurer's Accounts having been printed in the Circular calling the meeting, was held as read, and approved of. It was in substance as follows :—

ABSTRACT OF ACCOUNTS OF THE TREASURER.

SESSION 1871–72.

DR.

1871.—*Nov.* 1.				
To Balances from last Session—				
In Union Bank of Scotland, ...	£2	3	2	
In Treasurer's hands, ...	13	19	2½	
				£16 2 4½
Carry forward, ...				£16 2 4½

Brought forward, ...				£16 2 4½
1872.—*Oct.* 31.				
To Entry Money and Dues from 50 New Members, at 42s., .				105 0 0
,, Annual Dues from 4 Original Members, at 5s.,	£1	0	0	
,, Annual Dues from 489 Members, at 21s., .	513	9	0	
,, Annual Dues from 3 Members for two years, .	6	6	0	
				520 15 0
,, Call of 5s. towards expenses of Re-furnishing, 4 Members, at 5s., ...				1 0 0
,, Chemical Section.—1 Associate for 1869–70, 11 Associates for 1870–71, and 35 Associates for 1871–72, at 5s.,. . . .	£11	15	0	
Less Expenses, . .	5	3	10	
				6 11 2
,, Sanitary Section.—6 Associates for 1870–71, and 30 Associates for 1871–72, at 5s., . .	£9	0	0	
Less Expenses, . . .	3	16	10	
				5 3 2
,, Corporation of Glasgow, interest on "Exhibition Fund,"	£48	15	11	
,, Interest from Bank, ...	2	4	10	
				51 0 9
,, Catalogues sold, 32 at 1s., ...	£1	12	0	
,, *Proceedings* sold, ...	0	14	5	
				2 6 5
				£707 18 10½

Cr.

1872.—*Oct.* 31.				
By Salaries and Wages, ...				150 16 3
,, New Books and Binding, ...	£172	12	7	
,, Printing *Proceedings*, Circulars, &c., . .	125	18	6	
,, Postage and Delivery of Circulars, &c., . .	21	11	10	
,, Stationery, ...	3	1	3	
,, Lithographing Plates, &c., ...	12	6	6	
				335 10 8
,, Rents, ...	£130	0	0	
,, Insurance, Gas, Coal, Cleaning, &c., . . .	27	0	0	
,, Petty Charges and Sundries, ...	1	17	0	
				158 17 0
,, Subscriptions to Livingstone Search Expedition Fund, . .				20 0 0
,, ,, Ray Society, 1872, . . .	£1	1	0	
,, ,, Palæontographical Society, 1872, .	1	1	0	
				2 2 0
,, Architectural Section.—Expenses for year 1871,	£13	19	4½	
,, for year 1872,	10	3	6	
				24 2 10½
,, Balances—In Union Bank, ...	£14	4	10	
In Treasurer's hands, . . .	2	5	3	
				16 10 1
				£707 18 10½

GLASGOW, 13*th November*, 1872.—We, the Auditors appointed at a Meeting of the Society, held on 6th instant, have examined the Treasurer's Accounts, of which the above is an Abstract [condensed], and found them correct, the Balance in Union Bank at 31st October last being Fourteen pounds four shillings and ten pence sterling; and in Treasurer's hands, Two pounds five shillings and three pence.

(Signed) JAMES REID.
GEORGE WATSON.

REPORT BY THE LIBRARIAN.

The Librarian read the following Report by the Library Committee on the State of the Library, which was approved of.

During the year which has elapsed since the last occasion of your Library Committee reporting on the state of the Library, the alterations which were explained in the two preceding Reports as having been made in conducting the work of the Library have been continued, and found to operate satisfactorily,—these alterations giving at any moment a certain knowledge with respect to every book which the Society possesses. The result of which has been that, whereas in former years books were sometimes lost, there is not a single instance on record, with which your Committee are acquainted, of the permanent loss of any book since the alterations referred to were made. The Librarian has in every instance been able to trace out every apparent loss; and if he has not succeeded in recovering the original Library volume, he has yet obtained from the loser a new copy.

With respect to the purchase of new books during the past Session, your Committee can only express their deep regret that the maintaining the Library fully provided with the literature which has come from the press in that period has been impossible to them, by reason of it having been found by the Finance Committee necessary to limit the general expenditure, in order to place the Society free of debt; and this object having been nearly accomplished, it is expected that the sum hitherto expended will be available for the present and future Sessions. None are more sensible than your Library Committee of the importance of keeping the Library at all times provided with the most recent printed matter essential to the interest and necessity of its members; and whilst your Committee have occasion to remark with some satisfaction a more extended use of the "Proposal Book" by the members, they have to admit that, from the circumstances mentioned, it has not been in their power to comply with the recommendations in regard to the books so proposed. In the session twenty-one books, independently of forty-two periodicals, British and Foreign, have been

purchased, and twenty-six donations have been received,—so that sixty-three volumes have been added. For the donations the usual letters of acknowledgment have been sent to the donors. In binding, several arrears have been overtaken, seventy-seven volumes having been bound; whilst, in regard to exchanges, the Society has on its roll seventy different Societies and individuals to whom the *Proceedings* are sent, and from whom publications are received in return.

A discussion took place on a proposal made by several members for shortening the hours during which the Library is kept open. On the motion of Mr. George Watson, the subject was remitted to the Council, with a recommendation that the hours should be shortened, with a due regard to the convenience of the Society.

The Society then proceeded to elect four members of Council in room of Dr. J. G. Fleming, Mr. James Napier, Chemist, Mr. Daniel Macnee, and Mr. Robert Gray, who now retire by rotation. Mr. William Ramsay and Mr. James King consented to act as Scrutineers of Votes.

Several gentlemen were proposed and voted upon by ballot.

The Scrutineers reported that the following had been duly elected, viz:—

Mr. William M'Adam, Mr. Alexander Whitelaw, Dr. Thomas E. Thorpe, and Mr. Thomas Chapman.

Mr. James R. Napier gave an account of the Livingstone Relief Expedition, *via* the Congo, sent out at the expense of James Young, Esq. of Kelly. Mr. Napier having been furnished by Mr. Young with documents and a sketch map to enable him to make this communication, the Society agreed to record its thanks to Mr. Young.

OFFICE-BEARERS

OF THE

PHILOSOPHICAL SOCIETY OF GLASGOW,

1872-73.

President.

PROFESSOR ROBERT GRANT, M.A., LL.D., F.R.S.

Vice-Presidents.

PROFESSOR SIR WILLIAM THOMSON, D.C.L., LL.D., F.R.S., F.R.S.E.
PROFESSOR JOHN YOUNG, M.D., F.R.S.E.

Librarian.

MR. ST. JOHN VINCENT DAY, C.E., F.R.S.E.

Treasurer.

MR. JOHN MANN, C.A.,

Secretary.

MR. WILLIAM KEDDIE, F.R.S.E.

Other Members of Council.

MR. WILLIAM R. W. SMITH.	DR. ANDREW FERGUS.
MR. JAMES ANDERSON.	MR. DAVID ROWAN.
MR. HORATIO K. BROMHEAD, A.R.I.B.A.	DR. JAMES BRYCE.
	MR. THOMAS CHAPMAN.
DR. WILLIAM WALLACE, F.R.S.E., F.C.S.	DR. T. E. THORPE, F.R.S.E.
	MR. ALEXANDER WHITELAW.
DR. JAMES MORTON.	MR. WILLIAM M'ADAM.

December 4, 1872.—*The* PRESIDENT *in the Chair.*

The following were elected members, viz.:—

Mr. John Neil, Whitehill House, Dennistoun; Mr. Alexander Moir, Manufacturer, Mitchell Street; Mr. James Nicol, City Accountant, and Factor to Glasgow Improvement Trustees, City Chambers; Mr. George Morison, Stockbroker, 19 Royal Terrace; Mr. Robert Watson, Merchant, Hazelcliff, Cove, by Greenock; Mr. Robert M'Farlane, 28 Napiershall Street; Mr. David Fulton, Engraver, Duke Street Engraving Works; Mr. James Barclay, Merchant, 224 West George Street; Mr. David Robertson, Bookseller, 90 St. Vincent Street; Mr. John Clark, 9 Wilton Crescent; Mr. Paterson Wingate, Engineer and Shipbuilder, Broomhall, Partick.

Mr. St. John Vincent Day read a communication (the second) "On some Evidences as to the very early Use of Iron."

Some discussion followed, in which Mr. Jamieson, Mr. Walter Macfarlane, Mr. John Campbell, Dr. Thorpe, Mr. Gorman, Dr. Bryce, and others, took part.

December 18, 1872.—*The* PRESIDENT *in the Chair.*

The following were elected members, viz.:—

Professor George Forbes, B.A., F.R.S.E., Andersonian University; Mr. John Farquhar, Tower Bank, Lenzie; Dr. Hugh Miller, 3 Crescent Place; Mr. James Edward Stoddart, Chemist, Dalquhurn Dye Works, Renton; Mr. Archibald M. Lindsay, M.A., Writer, 87 West Regent Street; Mr. T. W. D. Gillies, Prince's Square; Dr. John Alexander Lothian, L.R.C.S.E., 37 Sauchiehall Street; Mr. George L. M'Pherson, 37 M'Auslan Street; Mr. Quintin Pringle, B.Sc. LL.B., Teacher, 66 Hamilton Place, Hillhead; Mr. John Swanston, Doubler, 1 Grafton Square.

Dr. Stevenson Macadam, Professor of Chemistry in the Royal College of Surgeons, Edinburgh, read a paper "On Flour-Mill Fire Explosions." The paper was brought before the Society by an arrangement with the Chemical Section. It was illustrated by numerous experiments. A vote of thanks was given to Dr. Macadam.

Professor Rankine, through the Secretary, expressed his great regret that the state of his health prevented his being present at the meeting. He had been employed by the Fire Offices along with Dr. Stevenson Macadam, to report on the causes of the destruction of the Tradeston Flour Mills and adjoining stores; and he was fully cognizant of all the facts of the case. As to opinions, he concurred in those which had been expressed by Dr. Macadam and himself in their joint report. The experiments on a small scale were contrived and carried out by Dr. Macadam alone; and he thought that they illustrated in a most striking way the inflammable and explosive properties possessed by solid combustible matter when diffused through air in the state of fine dust. A curious example of those properties was afforded by Mr. Crampton's method of feeding furnaces with coal dust blown in by a blast of air.

The Society adjourned for the holidays, to resume its meetings on the 8th of January, 1873.

January 8, 1873.—*The* PRESIDENT *in the Chair.*

The President, before the business of the meeting was entered upon, said,—Gentlemen,—Since our last meeting an event occurred which has deprived this Society of one of its most distinguished

Members, and has created a blank in the scientific world which will not be easily filled up. On the 22d of last month, William John Macquorn Rankine, Professor of Engineering in the University of Glasgow, breathed his last. On a future occasion it will be proper to bring before the Society a detailed account of the life and scientific career of this eminent man. In the meantime I would crave the indulgence of the Society in offering one or two remarks suggested by the lamented occurrence which has thus robbed science of one of its greatest ornaments. Professor Rankine was so generally known in this city, he had so endeared himself to all his acquaintance by his personal qualities, and he had thoroughly identified himself with so many matters which concerned the prosperity of Glasgow, that we can easily account in this manner for the gloom which the announcement of his death cast over our city. But the value of Rankine's labours, and the well-earned renown which they acquired for him, were not confined to Glasgow or Scotland, they extended to every country in the civilized world. Wherever mechanical science was studied as one of the great pioneers of modern progress; wherever the steam-engine was employed in changing the entire economy of labour, and in bringing distant nations into friendly intercourse with each other; wherever science was loved purely for its own sake, and irrespective of any social or material influence which it might exercise, there the name of Rankine was held in honour and esteem. To the University with which he was connected, and which he loved so well, his loss is all but irreparable. By his colleagues, who had ample means of becoming acquainted with his personal worth and his high qualities as a man of science, he is mourned as for a friend whom no mere shifting of circumstances by the lapse of time can replace. I would here mention one or two traits in the character of the late Professor Rankine, which could not fail to impress themselves on the mind of any one who enjoyed the privilege of his acquaintance. One of these was the thoroughness with which he worked out any object to which he devoted his attention. This is apparent in his writings, but it was also clearly evidenced by his conduct in all matters connected with the ordinary business of life. Another feature of his character which impressed me strongly was this, while he was not insensible to the esteem which his labours so justly secured for him, he despised the attempt of advancing his reputation by any of the artifices which are so common in our day. I would further remark, that in private life Professor Rankine gave practical proof of the circumstance that the most profound devotion to scientific pursuits is not incompatible with the playful

disposition, the quiet humour, and other genial qualities which render the possessor of them the delight of the social circle. As regards the scientific labours of this distinguished man, it is a subject which I have no intention to enter upon at present. I shall merely remark that his researches connected with the dynamical theory of heat would alone suffice to assure to him a permanent name in the annals of science. Finally, I would state that by the sterling uprightness of his character, and the whole tenor of his intellectual career, he has constituted an example worthy of being emulated by every genuine cultivator of science. In accordance with the custom of this Society, I would beg to propose that it be remitted to a Committee to prepare a Minute expressive of the deep sense which the Society entertain of the loss they have sustained by the death of Professor Rankine, and that the Minute be engrossed in the Minute-book of the Society.

On the motion of Mr. Mayer, seconded by Sir William Thomson, the thanks of the Society were voted to the President for his graceful tribute to the memory of Dr. Rankine.

The proposal of the President being approved of, Dr. Grant, Sir William Thomson, and Dr. Bryce were appointed a Committee.

The following were elected members of the Society, viz.:—

Mr. William Edward M'Laren, Steamship Owner, 141 Buchanan Street; Mr. Andrew Dow Dixon, Sharebroker, 19 Elmbank Crescent; Dr. Joseph Snape, M.R.C.S. Eng., L.R.C.P., & L.M. Ed., 184 West Regent Street; Rev. Thomas M. Lindsay, F.R.S.E., F.A.S., M.A., Professor in the Free Church College; Dr. James Finlayson, 351 Bath Street; Mr. Alexander Morrice, 27 Abbotsford Place; Mr. James Sellars, Jun., Architect, 266 St. Vincent Street; Mr. William Couper, 141 St. Vincent Street; Mr. John Anderson, 4 Franklin Terrace; Mr. Robert Gourlay, Bank of Scotland; Mr. Thomas Keay, Banker, Moore Place, West George Street; Mr. Thomas Scott, 1 North Ure Place; Mr. Robert Cameron, 61 Parliamentary Road; Dr. Hector C. Cameron, 294 Bath Crescent; Mr. William Dron, Engineer, Cranstonhill; Mr. John Wilson, 11 Woodside Place.

Mr. James R. Napier read a paper "On British Weights and Measures."

The paper was followed by some remarks by Sir William Thomson, and on the motion of Mr. Day the discussion was adjourned.

January 22, 1873.—*The* PRESIDENT *in the Chair.*

The following were elected members, viz:—

Mr. J. M. Ormond, M.A., The Park School, Lynedoch Street; Dr. Ebenezer Duncan, C.M., F.F.P.S.G., 3 Brougham Terrace, Crosshill; Mr. Thomas Wright, 128 Bothwell Street; Mr. John E. Jack, Bank Street, Hillhead.

The Society agreed to insert in the minute book the following minute relating to the death of Professor Rankine:—

The Philosophical Society of Glasgow records its profound sorrow for the loss it has sustained by the death of Dr. William John Macquorn Rankine, who was for twenty years one of its most distinguished members, and for the greater part of twelve years took an active interest in its management as one of its Office-bearers. His connection with the Society began in 1852; he became a member of the Council in 1854; was Vice-President in Sessions 1855-56, and 1858-59; was President in Sessions 1860–61 and 1861–62; and was again a member of Council for other three years.

This is not the occasion for estimating the magnitude and importance of his labours in the promotion of mechanical science, upon which his great reputation will long securely rest. But the Society feels a melancholy satisfaction in here recalling his principal contributions to its *Proceedings*, the more especially as he honoured the Society with early communications upon some of the researches which first made his name illustrious in the annals of science. These contributions were the following, viz:—"On the General Law of the Transformation of Energy;" "On the Determination of the Magnetic Meridian at a distance from Land;" "On the Approximate Determination of the Azimuth of a Star by Geometrical Construction, its Declination and Altitude, and the Latitude of the place of Observation, being given;" "Outlines of the Science of Energetics;" "On Telegraphic Communication between Great Britain and Ireland;" "On the Stability of Factory Chimneys;" "Report on the Progress and State of Applied Mechanics,"—the sequel of which afterwards formed the substance of the author's two Annual Addresses from the chair of the Society;—"On the Density of Steam;" "On the use of Mechanical Hypotheses in Science, and especially in the Theory of Heat;" "On the Estimation of the Evaporative Power of Fuel;" and finally, "On the Phrase, '*Potential Energy*,' and on the Definitions of Physical Quantities."

Services thus varied and invaluable may well impress the Society with an abiding sense of what it owes to the memory of Dr. Rankine,

and of its irreparable loss by his death. Nor can his former associates in the Council ever forget the cheerful alacrity with which he addressed himself to the business of the Society, the unalloyed pleasure they enjoyed in their intercourse with him, the habitual courtesy and geniality of his manners, and his unfailing kindness of heart.

The Society ordered that fifty copies of Mr. Napier's paper, "On British Weights and Measures," be printed for the use of members, on resuming the discussion of the subject at next meeting.

Mr. Mayer gave notice that at next meeting he would call attention to the Minutes passed in previous years as to issuing the printed *Proceedings* during the Session.

A letter was received from Mr. Lockhart, Secretary to the Committee for promoting the formation of a Technical College in Glasgow, requesting, on the part of the Committee, that a deputation of members of the Philosophical Society be appointed to confer with the Committee on the subject. The following were appointed, viz:—the President, Mr. David Rowan, Mr. William M'Adam, Dr. Wallace, and Mr. Alexander Whitelaw.

Professor Bischof read a paper "On the Purification of Water."

February 5, 1873.—*The* PRESIDENT *in the Chair.*

The following were elected members, viz.:—

Mr. George Gardner, Solicitor, 68 St. Vincent Street; Dr. Neil Carmichael, C.M., 81 South Portland Street; Mr. David Millar, 6 Royal Terrace; Mr. William Leishman Buchanan, Accountant, 14 Lynedoch Crescent; Dr. Robert Smart, 22 St. George's Road; Mr. John Craig, Merchant, 24 Hamilton Park Terrace.

The President gave in the following Report from the deputation appointed by the Society to confer with the Committee on the proposed Technical College, in compliance with an invitation from the Committee for the co-operation of the Society:—

The deputation appointed by the Society to confer with the Provisional Committee for establishing a Technical College in Glasgow, in response to an invitation from the latter requesting the co-operation of the Society, attended a meeting of the Committee at their rooms in Bath Street, on Thursday, the 23rd ultimo. Mr. Walter Montgomerie Neilson, the Chairman of the Committee, explained the objects of the proposed institution; and additional statements and

remarks were afterwards made by Mr. Wakefield, Mr. Sandeman, and several other gentlemen present. The deputation assured the Committee of the warm sympathy with which the Council of the Philosophical Society regarded any movement having for its object to elevate the character of the skilled labour required in the industrial works of Glasgow and the country generally; but in the meantime, they reserved the expression of any opinion in regard to the plan which it would be most expedient to adopt for attaining that end. After some conversation it was finally agreed upon that a paper on the subject of Technical Education should be read before the Philosophical Society in the course of the current session, when an opportunity would be afforded to the members of the Society of having a full discussion of the proposed scheme.

R. GRANT, CONVENER.

Mr. John Mayer having called attention to former resolutions of the Society to issue the printed *Proceedings* monthly, it was agreed, on the motion of Sir William Thomson, to revive the practice. The Society therefore ruled that the *Proceedings* shall be issued monthly during the Session, and sent to those members who shall indicate their desire to receive the monthly sheets by leaving their names at the Library, or by a note to the Secretary. All the members will, as usual, receive a complete copy of the *Proceedings* at the close of the Session.

The discussion on Mr. Napier's paper on "British Weights and Measures" was resumed by Mr. Day. A few remarks were afterwards made by Sir William Thomson and Mr. M'Gavin.

February 19, 1873.—*The* PRESIDENT *in the Chair.*

A minute of Council was read, stating that, on the recommendation of the Finance Committee, the Council had unanimously agreed to recommend the Society to add £25 per annum to the Secretary's salary of £50, in consideration of the addition to his duties caused by the increase of the Society.

It was moved by Mr. W. R. W. Smith, and seconded by Mr. Whitelaw, that the recommendation of the Council be approved of by the Society; which motion, having been submitted to the meeting, was agreed to.

A paper "On Numeral and Metral Systems," by E. L. Garbett, Esq., was communicated to the Society by Mr. James R. Napier. The thanks of the Society were voted to Mr. Garbett for his paper.

March 5, 1873.—The PRESIDENT *in the Chair.*

The following were elected members, viz.:—

Mr. William Thomson, Insurance Manager, 89 Buchanan Street: Mr. James Graham Smith, 13 Park Circus Place, Park Street, East; Mr. George Simpson, C. and M. Engineer, 170 Hope Street.

The Society, by a second vote, agreed to the recommendation of the Council that £35 be added to the Secretary's salary.

A paper was read by Mr. W. R. W. Smith "On the Present Aspect of the Sewage Question, and what Glasgow should do in relation to it."

March 12, 1873.—The PRESIDENT *in the Chair.*

Mr. Andrew Symington, Wine Merchant, 119 St. Vincent Street, was elected a member of the Society.

Professor Forbes read a paper "On the Transit of Venus in 1874."

April 9, 1873.—PROFESSOR YOUNG, M.D., *Vice-President, in the Chair.*

Mr. William Miller, Banker, 7 Victoria Terrace, Dowanhill, was elected a member.

The Vice-President called the attention of the Society to a set of the publications of the Royal Observatory, Greenwich, presented to the Library of the Society by the Astronomer-Royal, by authority of the Lords Commissioners of the Admiralty, and moved that a special vote of thanks be given to the Astronomer-Royal for this very valuable gift.

The motion was agreed to by acclamation; and the Secretary was instructed to communicate the resolution of the Society to the Astronomer-Royal.

A letter was received from Mr. Edmund Hunt, mentioning that the captain and officers of H.M. ship "Malabar" are to be stationed in Glasgow during the greater part of the summer, whilst that vessel is in the hands of Messrs. Robert Napier & Sons, and suggesting that the Society might shew them the courtesy of inviting them to attend any of the remaining meetings of the Session.

The Society cordially approved of Mr. Hunt's suggestion, and agreed also to offer these gentlemen free access to the Library during their residence in Glasgow.

Mr. David Sandeman read a paper on "Technical Education,"

which was followed by a statement on the same subject by Mr. W. Montgomerie Neilson.

In the discussion which followed, Mr. James Napier, Mr. E. Jacoby, Dr. Bryce, and Dr. Young took part.

April 23, 1873.—The PRESIDENT *in the Chair.*

Mr. Robert Macintyre, Brassfounder, 3 Hill Street, Garnethill, was elected a member.

The following were proposed as members, to be balloted for at the opening of next Session, viz.:—

Mr. John Boyd, Engineer, Nassau Works, Bishop Street; Mr. Peter M'Intyre, 122 Brunswick Street.

The President stated that, at the request of the Council, he had asked the opinion of the Senatus of the University as to the propriety of sending deputies to the ensuing meeting of the British Association, to renew the invitation to that body to hold an early meeting in Glasgow; and that the Senatus considered it inexpedient at present to take any steps in the matter.

Mr. Robert R. Tatlock gave in the following

REPORT OF CHEMICAL SECTION.

THE Council of this Section are glad to be able to state that the Session now closing has been successful in the highest degree Nine new Associates have been enrolled, and eighteen papers read on important and interesting subjects,—mostly of a practical character.

The following are the titles of the communications:—

1872.

Nov. 11. Introductory Lecture on "Pure Air, Pure Water, and Pure Food," by DR. WALLACE, F.R.S.E., President.

„ 25. Business Meeting.

Dec. 9. "On the Volcanic Vapours of Vesuvius," by JAMES ANDERSON, Esq.

„ 18. (Conjoint Meeting), "On Flour-Mill Fire Explosions," by DR. STEVENSON MACADAM, F.R.S.E., Edinburgh.

1873.

Jan. 13. "On the Composition of some Zeolites," by J. WALLACE YOUNG, Esq.; and "On the Decomposition of Sulphate of Potash by Nitrate of Soda," by WM. HENDERSON, Esq.

Jan. 27. "On the Life and Works of Christopher Glaser," by JOHN FERGUSON, Esq., M.A.

Feb. 10. "On the Analysis of Glasgow Whisky," by ROBERT R. TATLOCK, Esq., F.R.S.E., F.C.S.

„ 24. "On the Chemical History of Anthracene and its Production from Coal Tar," by R. F. SMITH, Esq.

March 10. "On a Simple Method of Accelerating Filtration," and "On a New Method for the Estimation of Nitric, Chloric, and Iodic Acid," by DR. T. E. THORPE, F.R.S.E.; "On the Mineral Constituents of Sound and Diseased Potatoes," by JAMES B. HANNAY, Esq., F.C.S.; and "On Toluic Acid," by WM. RAMSAY, Esq.

„ 24. "On two New Methods of Estimating Mercury," &c., by JAMES B. HANNAY, Esq., F.C.S.; and "A Description of Soleil's Polarizing Saccharometer, as lately improved by the addition of Jellet's Prism," by T. L. PATTERSON, Esq., F.C.S.

April 14. "On the Mortar Employed in the Building of the Great Pyramid," "On Lake Deposits from Central Asia," and "On the Apparatus of Professor Klinkerfues for the Instantaneous Lighting and Extinguishing of Street Lamps," by DR. WALLACE, F.R.S.E., President.

The Council respectfully invite the members of the Parent Society to the Chemical Section's meetings on all occasions.

ROBERT R. TATLOCK,
Secretary of Section.

Mr. Gavin Irving Dickson gave in the following

REPORT OF THE SANITARY AND SOCIAL ECONOMY SECTION.

THE Council have to report another active and interesting Session.

The following is an abstract of the proceedings:—

1st Meeting.—Inaugural Address of the Session by the President, Dr. Fergus, on "Private Bill Legislation—its Evils and the Remedy."

2nd, 3rd, and 4th Meetings.—Paper by Mr. David Carnegie, of Stronvar, Lochearnhead, on the "Swedish Laws for Regulating the Trade in Spirits, and on the System adopted in Gothenburg for the Sale thereof."

5th Meeting.—Paper by Mr. J. L. Lang, Writer, Glasgow, on "Free Libraries, Museums, and Reading-Rooms in England, America, the European Continent, and some towns in Scotland. Why not in Glasgow?"

6th Meeting.—Paper by Mr. Alexander Scott, Glasgow, on "Some General Deductions from the present position of the Vital Statistics of Glasgow, in reference to recent changes affecting the same."

7th and 8th Meetings.—Papers by Messrs. Stephen Mason, Wm. Miller, and James Anderson, on "Banking and Currency."

9th Meeting.—Paper by Mr. John Dougall, M.D., Medical Officer of Health for the Burgh of Kinning Park, on "The Dissemination of Zymotic Diseases by Milk."

10th Meeting.—Paper by Town Councillor Durham, of Edinburgh, on "Moral Obligation of the Government to Abolish the Excise Duty on Spirits and its consequent dealing with the present Licensing System."

The attendance at the various meetings was very encouraging, and the discussions on the different Papers were maintained with considerable animation and interest.

There are now thirty-four Associates of the Section; and the Council trust that the general members of the Society will largely add to their number, during the next Session, by inducing their younger friends to join the same.

The present members of the Council are as follows:—

Mr. Andrew Fergus, M.D., *President.*
„ W. R. W. Smith, *Vice-President.*
„ William M'Adam, *Vice-President.*

Professor E. Caird, M.A.	Mr. Robert Renfrew, M.D.
Mr. William Teacher.	„ Arthur Herriot.
„ William Walker.	„ John M'Intyre.
„ D. G. Hoey, C.A.	„ Stephen Mason.

Mr. Alex. Scott.

GAVIN IRVING DICKSON,
Secretary of Section.

Dr. Robert Bell read a paper on "The Potato and its Disease," illustrated by the Lime Light. Mr. James Anderson, Mr. W. R. W. Smith, and others, took part in the discussion of the paper.

A paper "On the Archæology of Cast Iron," by Mr. St. John Vincent Day, was held as read.

The President then intimated the close of the Session.

On the motion of Mr. Sandeman, the Society voted its cordial thanks to Dr. Grant for his services in the Chair during the Session.

ADDITIONS TO THE LIBRARY.

Donations.	Presented by	
Annual Report of the Royal Cornwall Polytechnic Society, 1871, . .	The Society.	
Proceedings of the South Wales Institute of Engineers, 1872, ...	The Institute.	1 Vol.
Journal of the Royal Institution of Cornwall, No. XIII., 1872, . . .	,,	
Statistics of the Colony of Victoria, . } Patents and Patentees for 1870, . . }	Regist.-Gen., Victoria.	7 Parts. 1 Vol.
Proceedings of the Literary and Philosophical Society of Liverpool, 1870-71,	The Society.	1 Part.
Journal of the Anthropological Society,	,,	1 ,,
Extracts from the Opening Address of the President of the Botanical Society of Edinburgh, 1870, ...	Sir W. Elliot.	Pamphlet.
Newport Puddling Furnace, . .	Jeremiah Head.	,,
Journal of the Franklin Institute, .	The Institute.	4 Parts.
Mineral Statistics of Victoria for 1871,	Wm. M'Lellan.	1 Part.
Etude sur les Races Indigénes, 1872, .		1 ,,
Memoires de la Société des Sciences, .	The Society.	1 ,,
Proceedings of the Philadelphia Academy of Natural Sciences, 1871, . .	,,	1 Vol.
Annals of the New York Lyceum of Natural History, ...	,,	1 Part.
Naval Observatory, United States, Washington, Astronomical and Meteorological Observations made during the year 1869, ...	B. F. Sands.	Pamphlet.
Proceedings of the California Academy of Sciences, ...	A. Kellogg.	6 Parts.
Proceedings of the Bath Natural History and Antiquarian Field Club, . .	The Society.	1 Part.
Transactions of the Institution of Engineers in Scotland, 1872, . .	The Institution.	1 Vol.
Proceedings of the Geological and Polytechnic Society, West Riding of Yorkshire, ...	The Society.	1 Part.
Proceedings of the Geological Society, Liverpool, 1871-72, ...	,,	1 ,,
The Annual Report of the Leeds Philosophical and Literary Society for 1871-72, ...	,,	Pamphlet.

Donations.	Presented by	
Monthly Notices of the Royal Astronomical Society,	The Society.	
Paisley Free Library and Museum,	Rev. Dr. Fraser.	1 Vol.
What Determines Molecular Motion,	James Croll.	Pamphlet.
Retail Traders and Co-operative Stores,	Jeremiah Head.	,,
Hutton's Tracts, 1812,	John Morgan.	3 Vols.
The Licensing Laws of Sweden,	D. Carnegie.	Pamphlet.
Annual Report of the Health of the City for 1872,	K. M. Macleod.	Pamphlet.
Transactions of the Society of Engineers,	G. W. Harris.	1 Vol.
Census of Victoria,	Registrar-General.	2 Parts.
Notes and Reminiscences relating to Partick,	James Napier.	1 Vol.
Outlines of the Natural History of Europe, &c., 1852,	J. D. Campbell.	1 ,,
Proceedings of Cleveland Institution of Engineers for 1872,	The Society.	1 Part.
A New System of Nature, on the Basis of the Holy Scriptures. William Hamilton Stewart, 1861,	James Eadie.	2 Vols.
Steam in the Engine; its Heat and its Work,	P. Kauffer.	Pamphlet.
Canadian Entomologist,	E. B. Reed.	1 Vol.
Papers relating to the Transit of Venus in 1874,	B. F. Sands.	Pamphlet.
On the Right Ascensions of the Equatorial Fundamental Stars. By Simon Newcomb, 1872,	,,	,,
Greenwich Observations from 1836 to 1870,	Royal Observatory, Greenwich.	33 Vols.
Greenwich Observations, Reduction,	,,	4 ,,
,, Magnetical and Meteorological Observations,	,,	4 ,,
Verification and Extension of La Caille's Arc of Meridian,	,,	2 ,,
Tables de la Lune, par P. A. Hansen,	,,	1 ,,
Greenwich Magnetical and Meteorological Results, from 1849 to 1870,	,,	22 Parts.
Greenwich Astronomical Results, from 1847 to 1870,	,,	24 ,,
Appendix to Greenwich Observations,	,,	5 ,,
Greenwich Catalogue; 2,760 Stars for 1864,	,,	1 Part.
Paramatta Catalogue; 7,385 Stars in the years 1822 to 1826,	,,	1 ,,
Observations at the Royal Observatory, Cape of Good Hope, by Sir Thomas Maclear,	,,	1 ,,
Report of the Total Eclipse of the Sun, August 17, 18, 1868,	,,	1 ,,

Donations.	Presented by	
Eiffe's Improvements in Chronometers, and Molyneux's Specifications of Patents for Improvements in Chronometers, 1842, ...	Royal Observatory, Greenwich.	1 Part.
Longitude of Valentia, ...	,,	1 ,,
,, ,, by Galvanic Signals in the Summer, 1862, . .	,,	1 ,,
Astronomical Observations made at Williamson's Observatory, in 1861–63,	,,	1 ,,
Results of Astronomical Observations made at the Royal Observatory, Cape of Good Hope, in 1856, . . .	,,	1 ,,
Lecture on the Greenwich System of Time Signals, Feb. 24, 1865, . .	,,	1 ,,
Maskelyne's Ledger of Stars, . .	,,	1 ,,
Reduction of the Observations of the Deep-sunk Thermometers of the Royal Observatory, Greenwich, . . .	,,	1 ,,
Breen's Correction of Bauvard's Elements of Jupiter and Saturn, . . .	,,	1 ,,
Report on the Teneriffe Astronomical Experiment of 1856, ...	,,	1 ,,
Reduction of the Greenwich Planetary Observations, 1831 to 1855, . .	,,	1 ,,
Bessel's Refraction Table, Modified and Expanded, 1853, ...	,,	1 ,,
Apparent Right Ascension of Polaris and β Urse Minoris, &c., 1846, . .	,,	1 ,,
Account of the New National Standard of Length and of its Principal Copies,	,,	1 ,,
Plan of the Building and Grounds of the Royal Observatory, 1863, . .	,,	1 ,,
Biographical Notice of the late John Scouler, M.D., LL.D., . . .	Wm. Keddie.	Pamphlet.
Bulletins de l'Académie, . . .	A. Quetelet.	4 Vols.
Annuaire de L'Academie Royale, 1872-73, Bruxelles, ...	,,	2 ,,
Académie Royale de Belgique; Centième Anniversaire de Fondation, 1772-1872, ...	,,	2 ,,
Annales Meteorologiques, 1871, . .	,,	1 Part.
Observation des l'hénomènes Périodiques pendant l'année, 1870, . . .	,,	1 ,,
Tables de Mortalité, Bruxelles, 1872, .	,,	1 ,,
Kongliga Svenska Vetenskaps-Akademiens Handlingar, 1869. Stockholm,	Royal Acad., Stockholm.	3 Vols.
Öfversigt af Kongl. Vetenskaps-Akademiens Förhandlingar, 1867, . .	,,	2 ,,

Donations.	Presented by	
Lefnadsteckningar öfver Kongl. Svenska-Vetenskaps-Akademiens efter ar 1854. Aflidna Ledamöter. Stockholm, 1869,	Royal Acad., Stockholm.	1 Part.
Meteorologiska Jakttagelser Sverige, .	,,	3 ,,
Jaarboek van de Koninklijke Akademie van Wetenschappen, 1871, . .	,,	1 Part.
Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, 1872, ...	,,	2 Vol.
Processen Verbaal Koninklijke Akademie van Wetenschappen te Amsterdam, 1871, 1872, ...	Amsterdam Society.	1 Part.
Verhandelingen der Koninklijke Akademie van Wetenschappen, 1872, .	,,	1 ,,

LIST OF BOOKS BOUGHT.

Health and Comfort in House Building. By J. Drysdale, M.D., and J. W. Hayward, M.D. 12mo. 1872.

A Sketch Map of the Lake Region of Eastern Africa, shewing Dr. Livingstone's Discoveries and Routes, from 1866–1872. By Keith Johnston, F.R.G.S. 1872.

Fourth Report of the Commissioners appointed in 1868 to enquire into the best means of Preventing the Pollution of Rivers. 1872.

The Ibis. Parts 5, 6, and 7.

National Gazetteer of Great Britain and Ireland. Edited by N. E. S. A. Hamilton, F.R.S.L. 5 parts. 1872.

Gymnoblastic or Tubularian Hydroids. By C. J. Allman, M.D. 1872. Ray Society.

Mineral Statistics of the United Kingdom of Great Britain and Ireland for the year 1871. 8vo. 1872.

How I found Livingstone in Central Africa. By H. M. Stanley. 8vo. 1872.

Corals and Coral Islands. By James D. Dana, LL.D. 8vo. 1872.

Encyclopædia of Chronology. By B. B. Woodward, B.A., and William L. R. Cates. 8vo. 1872.

Forces of Nature: a Popular Introduction to the Study of Physical Phenomena. Translated from the French by Mrs. Norman Lockyer. 8vo. 1872.

Ancient Stone Implements, Weapons, and Ornaments of Great Britain. By John Evans, F.R.S., F.S.A. 8vo. 1872.

Air and Rain: the Beginnings of a Chemical Climatology. By Robert Angus Smith, Ph.D., F.R.S., F.C.S. 8vo. 1872.

Iron as a Material of Construction. By William Pole, F.R.S. 12mo. 1872.

Forms of Water in Clouds and Rivers, Ice and Glaciers. By John Tyndall, LL.D., F.R.S. 12mo. 1872.

Manual of the Education Act for Scotland. By Alexander Craig Sellar, Advocate. 8vo. 1872.

The Physical Geology and Geography of Great Britain. By A. C. Ramsay, LL.D., F.R.S. 8vo. 1872.

Michael Faraday. By J. H. Gladstone. 12mo. 1872.

Journey through the Caucasus and the Interior of Persia. By Augustus H. Mounsey, F.R.G.S. 8vo. 1872.

Beginnings of Life. By Charlton Bastian, M.A., M.D., F.R.S. 1872.

Chemical Phenomena of Iron Smelting. By J. Lowthian Bell. 8vo. 1872.

Reports, 2, 3, and 4 of the Warden of Standard Weights and Measures.

Journey Across South America. By Paul Macroy. 1872.

Statesman's Year Book: Handbook for Politicians and Merchants for the year 1873. By Frederick Martin. 12mo.

The Expression of the Emotions in Man and Animals. By C. Darwin, M.A., F.R.S. 8vo. 1872.

A Treatise on the Metallurgy of Iron. H. Bauerman, F.G.S. 12mo. 1872.

Treatise on the Building and Ornamental Stones of Great Britain and Foreign Countries. By E. Hull, M.A., F.R.S. 8vo. 1872.

Papers on Electrostatics and Magnetism. By Sir William Thomson, D.C.L., LL.D., F.R.S., F.R.S.E. 8vo. 1872.

Reports and Speeches on Local Taxation. By George J. Goschen, M.P. 1872.

Treatise on the Theory of Friction. By John H. Jellett, B.D. 8vo. 1872.

Manual of Chemical Analysis and Assaying. By L. L. de Koninck. 1872.

Physics and Politics. By Walter Bagehot. 1872.

Royal Society's Catalogue of Papers. 1872.

Journal of the Royal Geographical Society for 1872.

Philosophical Transactions of the Royal Society of London. Part I.

Depths of the Sea. By Wyville Thomson. 8vo. 1873.

Eruption of Vesuvius in 1872. By Professor Luigi Palmieri. 1873.

Life and Adventures of John James Audubon, the Naturalist. By Robert Buchanan. Third Edition. 8vo. 1869.

Archæological Sketches in Scotland, District of Kintyre. By Captain T. P. White. 1873.

Santo Domingo, Past and Present, with a Glance at Hayti. By Samuel Hazard. 1873.

Popular Scientific Lectures. By H. Helmholtz. Translated by E. Atkinson, Ph.D., F.C.S. 1873.

Manual of Palæontology, for the use of Students. By Henry Alleyne Nicholson, M.D., D.Sc., M.A., Ph.D., F.R.S.E. 1872.

Switches and Crossings. By William Donaldson, M.A., A.I.C.E. 1871.

Introduction to the Study of Palæontological Botany. By John Hutton Balfour, A.M., M.D., Edin., F.R.S., Sec. R.S.E., F.L.S. 1872.

Life and Letters of James David Forbes, F.R.S. Edited by John Campbell Shairp, LL.D., Peter Guthrie Tait, M.A., and A. Adams-Reilly. 1873.

Handbook of Hardy Trees, Shrubs, and Herbaceous Plants. By Decaisne, Naudin, and W. B. Hemsley. 8vo. 1873.

Lahore to Yarkand. By G. Henderson, M.D., F.L.S., F.R.G.S., and Allan O. Hume, C.B., F.Z.S. 1873.

Acadian Geology. By J. W. Dawson. 1868.

Philosophical Transactions of the Royal Society of London. Part II. for 1871.

Electricity and Magnetism. By Clerk Maxwell. 2 vols. 1873.

Life of A. Von Humboldt. By Bruhns and Lassell. 2 vols. 1873.

Digest of the English Census of 1871. By James Lewis. 1873.

Our Seamen: an Appeal. By Samuel Plimsoll, M.P. 1873.

LIST OF WEEKLY, MONTHLY, AND QUARTERLY PERIODICALS RECEIVED IN THE PHILOSOPHICAL SOCIETY'S LIBRARY.

Weekly.

Journal of the Society of Arts.
Pharmaceutical Journal.
Polytechnisches Journal.
Annales Industrielles.
Notes and Queries.
Chemical News.
Comptes Rendus.
Les Mondes.
Building News.
Engineering.
Athenæum.
Engineer.
Architect.
Builder.
Nature.
Iron.

Monthly.

The Country Gentleman's Magazine.
Annalen der Chemie und Pharmacie.
Annales de Chimie et de Physique.
Annalen der Physik und Chemie.
Journal de Pharmacie et de Chimie.
Annales des Ponts et Chaussees.
Journal fur Praktische Chemie.
Monthly Microscopical Journal.
Journal of the Chemical Society.
Geological Magazine.
Journal of Botany.
Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin.
Abstracts of the Proceedings of the Geological Society of London.
Monthly Notices of the Royal Astronomical Society.
London, Edinburgh, and Dublin Philosophical Magazine.
Annals and Magazine of Natural History.
American Journal of Science and Art.

Quarterly.

Bulletin de la Société Chimique de Paris.
Journal of the Iron and Steel Institute.
Journal of the Statistical Society.
Quarterly Journal of Science.
Journal of Anthropology.
Quarterly Journal of Microscopical Science.
Quarterly Journal of Geological Society.
Annales des Mines.

Philosophical Society of Glasgow. Exchange with other Societies, &c.

North of England Institute of Mining Engineers, . .	Newcastle-on-Tyne.
Institution of Mechanical Engineers, ...	Birmingham.
Midland Institute Mining Engineers, ...	Barnsley.

Canadian Institute	Canada.
Academy of Sciences	Missouri.
History Society	Iowa.
California Academy	California.
Professor S. L. Loomis	Connecticut.
Professor L. Loomis	"
Natural History Society	Portland, Maine.
Academy of Sciences	Philadelphia.
Franklin Institute	"
Pharmaceutical Society	"
American Philosophical Society	"
Boston Society	Boston.
American Academy	"
Boston Natural History Society	"
New York Agricultural Society	New York.
American Institute	"
Lyceum of Natural History	"
Professor J. Henry, National Observatory	Washington.
Commissioners of Patents	"
Smithsonian Institution	"
National Academy	"
United States Observatory	"
" " Survey	"
Soc. Hist. Nat. La Société des Sciences Physiques et Naturelles	Bordeaux.
Society of Physical Sciences	"
Geological Survey Office	Calcutta.
Wm. H. Archer, Registrar-General	Melbourne.
Royal Academy of Sciences	Berlin.
" " "	Stockholm.
" " "	St. Louis.
" " "	Brussels.
" " "	Amsterdam.
Royal Society of Victoria	Melbourne.
" Institute of Lombardy	Milan.
" Geographical Society	Vienna.
Geological Survey of India	Calcutta.
University of Christiania	Christiania.
Royal Institution of Cornwall	Truro.
Berwickshire Naturalist's Club	Alnwick.
C. J. Matthes, of the Royal Academy	Amsterdam.
E. B. Reed, Barrister-at-Law	London, Ontario.

INDEX.

PAGE

Anderson, Mr. J., on Vapours of Vesuvius, ... 268
Anderson, Mr. J., on the Mint Bank-Note of the Future, ... 417
Astronomy of the Nineteenth Century, ... 39

Bank Acts and the Rate of Discount, ... 443
Bank-Notes, Issue of, ... 394
Bell, Dr. Robert, on Biogenesis, . 101
Biogenesis, as bearing upon Putrefaction, &c., ... 101
Bischof, Prof., on Purification of Water, ... 357

Clark, Dr. John, on Chrome Ore, 174
Coal, Duration of, ... 9
Cochrane, Mr. M. H., on Jargons and Zircons of Ceylon, ... 186
Chrome Ore, Analysis of, ... 174

Day, Mr. St. John Vincent, on Early Use of Iron, ... 235
Day, Mr. St. John Vincent, on Iron Smelting, ... 464
Decomposition of Sulphate of Potash by Nitrate of Soda, ... 457
Double Sulphide of Gold and Silver, ... 184
Drainage and Sewerage of Towns, 112

Evolution Hypothesis, ... 17

Fehling's Copper Solution, ... 177
Flour-Mill Fire Explosions, ... 280
Forbes, Prof. G., on Transit of Venus in 1874, ... 373

Garbett, Mr. E. L., on Numeral and Metral Systems, ... 329

Henderson, W., on the Decomposition of Sulphate of Potash by Nitrate of Soda, ... 457

Iron, Early Use of, ... 235

PAGE

Iron Smelting, Past and Present of, ... 464

Jargons and Zircons of Ceylon, ... 186

Library, Librarian's Reports on, 212, 485
Library, Additions to, ... 499
Livingstone Relief Expedition, ... 278

Macadam, Dr. Stevenson, on Flour-Mill Fire Explosions, ... 280
Mason, Mr. S., on the Bank Acts and the Rate of Discount, ... 443
Miller, Mr. W., on Issue of Bank-Notes, ... 394
Mint Bank Note of the Future, ... 417
Minutes of Society, ... 208, 481
Muir, Pattison, Mr. M. M., on the Action of Dilute Saline Solutions upon Lead, ... 184
——— on Double Sulphide of Gold and Silver, ... 184

Napier, Mr. Jas. R., on Pressure Logs, ... 146
——— on Livingstone Relief Expedition, ... 278
Weights and Measures, ... 288
——— on British Numeral and Metral Systems, ... 329

Ogilvie, Mr. T. R., on Separation of Phosphoric Acid, Ferric Oxide, &c., ... 189

Patterson, Mr. T. L., on Fehling's Copper Solution, ... 177
——— on Superphosphate and Mineral Phosphates, ... 194
Phillips, Mr. John, on Drainage and Sewerage, ... 112
Phosphates, Mineral, &c., ... 194
Phosphoric Acid, &c., Separation of, ... 189

PAGE

Brunonian, Dr. Bryce, Opening Address ... 1
——— Professor Grant, Inaugural Address ... 31
Bruno, Law for Measuring the Speed of Sound ... 467
Progress of Science ... 1
Bryce, Mr. James, Obituary Notice of ... 1

Lecture, Prof. W. J. Macquorn Rankine, on Steam Hill Explosions ... 448
Lecture, Prof. W. J. Macquorn Rankine, Notes of his Lecture ... 448
Lecture, Prof. W. J. Macquorn Rankine, Minute recording his Lecture ... 449
Gladstone, Mr. J., on Technical Education ... 49
Scott, Mr. Alex., on The Institute of Glasgow ... 76
Section of Architecture, Report on ... 497
——— Chemistry, Report on ... 491
——— Economy and Social Economy, Reports on ... 496
Solar Eclipse, Recent, ... 160
State of the Society, Council's Reports on ... 208, 482

Technical Education, Mr. J. Gladstone on ... 49
——— Lectures on, by Mr. W. Montgomerie Neilson ... 451
——— Lectures on, by Professor Young, M.D. ... 41
Technical Education, Lectures on, by Mr. James Napier, Chemist ... 457
——— Report of Deputation of Society to Committee on ... 49

Thorpe, Dr., on Solar Eclipse ... 160

Venus, Transit of, in 1874 ... 1
The Institute of Glasgow ... 76
Volcanic Vapours of Vesuvius ... 242

Water Purification of ... 1
Weights and Measures, Imperial ... 1
——— Inaugural ... 307
——— Lectures on, by Sir William Thomson, 307
——— Lectures on, by Mr. Lay ... 1

BELL AND BAIN, PRINTERS, 41 MITCHELL STREET, GLASGOW.

Stanford University Libraries
3 6105 124 460 432

Stanford University Libraries
3 6105 124 460 432